

Lente (F.D.)

THE NEUROTIC ORIGIN OF DISEASE,

AND

THE ACTION OF REMEDIES ON THE NERVOUS SYSTEM.

BY

FREDERICK D. LENTE, M.D.,

OF COLD SPRING,

*Member of the New York Neurological Society; of the New York County Med. Soc.;
of the Board of Managers of the Hudson River State Hospital; of the Am.
Public Health Association; Cor. Member of the Med.-Legal Soc.
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Mr. President and Gentlemen :

MORITZ ROMBERG, Professor of Medicine in the University of Berlin, commences the preface to the first edition of his classical work on "Diseases of the Nervous System" with the following quotation: "I fear it will be a long time before combined efforts will enable a medical author to arrange and accurately describe the diseases of the nervous system. The position we at present occupy is a very inferior one. Such was the opinion of Sir Charles Bell. A decennium has since passed. Many laborers have been at work. We have a rich treasury of materials, and yet we possess no work on nervous diseases which meets the requirements of science. The blame lies, in a measure, with the distinguished members of our profession who have been deterred by a fear that pathological investigations would fail to cope with the advanced state of physiological inquiry; in others, the fault is to be attributed

to that mental indolence which gives the preference to the easy path of tradition, and with foolish skepticism rejects everything that is new. But in no department of pathology has physiology exerted so great an influence, nowhere has free research achieved so glorious a victory over dull, traditional routine, as in the doctrine of nervous diseases."

This was in 1840. Let us see what he says ten years later, for he thus indicates the history of our progress in this direction: "The hope expressed in a former edition has only been partly fulfilled. The majority of students have been attracted by the school which seeks to base the science of medicine exclusively upon pathological anatomy and chemistry. This has given rise to new errors, as the doctrine of the crases most clearly shows. The study of nervous diseases, which some persons have refused to acknowledge as anything but the manifestation of other morbid processes, has been declared a fruitless research, and in some schools has been almost interdicted. If this was the case in the universities, matters were necessarily in a worse condition in daily life. Practitioners chased an illusion and caricature called spinal irritation, with which they satisfied their craving for explanation, and condensed neuropathology into a space which could be covered with the tip of the finger. To guard against greater debasement, we must enter again upon the path which the master-mind of Charles Bell, the Harvey of our century, has opened to us."

If this was the case in critical and classical Germany, how much more true was it in the case of other countries? A quarter of a century has rolled by since Romberg thus rather sorrowfully expressed himself. Where are we now? We are certainly not where the wonderful improvements in every other branch of medical science should have placed us. But the past few years have afforded indications of a brighter future. Minds of the highest calibre have been devoting themselves to this study. Monographs, treatises, and periodicals, in different languages and of great value, have appeared to entice us on, and to light us along this difficult but most interesting path of scientific investigation; none more interesting, none more practically useful, in the estimation of those most competent to

judge, both in this country and in Europe, than that of our countryman, Prof. Hammond. But the teaching of our schools, and our systematic treatises on therapeutics, and our hospital clinics, are far behind our special literature. Their pathological views, as a general rule, show but little change. Mr. Wallace Wood, in a well-written article in the last number of the *Psychological Journal*, indulges in the hope that "the next five years will inaugurate a new system, which will promise as much or more than the brilliant but premature scheme of Gall." I think we may safely predict a future like this for the therapeutics as well as for the physiology of the nervous system.

For some years past my own experience has impressed me with the belief that in most diseases, especially those of an acute character, the morbid impression is first made on the nervous system, either *directly*, as in the case of the mucous membranes, where the peripheral nerves are more accessible to the immediate action of toxic agents; or *indirectly*, through the medium of the blood. That the remedies, therefore, most likely to exert an *abortive* effect on disease; or, when that is not attainable, the speediest and happiest relief, are those which are supposed to act especially on the nervous system; and that our ordinary treatment, by acting in a more indirect, and, therefore, less speedy manner, as well as by disturbing other portions and other organs of the body not involved in the morbid process, is slower, less certain, and more unpleasant in producing their curative effects. I ask your attention, in the first place, to a review of the history and therapeutics of certain diseases not commonly considered neuroses.

And, first, of "malarious" fever, and especially the intermittent form, as this is a disease of paramount importance to very many of us just now; its unusual prevalence everywhere, but especially in localities commonly considered remarkably exempt, being one of the most remarkable of the inexplicable phenomena characterizing its history in all ages.

I have no theory as to the causation of "malaria" to support. I am as much confounded by its vagaries and seeming inconsistencies as any of its hosts of distinguished investigators. But whatever the cause may be, whether vegeto-aerial or diurnal

vicissitudes of temperature and moisture, or both combined, or gaseous products, or the fungous theory, so ingeniously and learnedly set forth by the late J. K. Mitchell, of Philadelphia, it seems to act primarily on the nervous system, and its morbid energy to be most expended in assaults on that system.¹ The evidence adduced, both from the arguments and data of those adopting a certain theory of causation, and of those virulently opposing it, alike tends to support this idea. Thus it is admitted, I think, by all, that *repose*, and especially *repose at night*, renders the body most liable to miasmatic influences; while exercise, even in what is supposed to be the hotbed of the poison, is preventive. Every one knows how much more liable we are to "take cold" when sleeping, or even when in a drowsy state, and especially at night, unless the body has an additional covering, and how great a protection exercise is. In both cases the nervous system is more impressible in the one state than in the other. Exercise and occupation, in all *epidemics* of whatever kind, are the great preventives; indolence and fear, causing greater nervous impressibility, are our worst enemies. All army surgeons of large experience can corroborate this.

The sudden onset of the disease, instances of which abound in all their writings, point to the primary affection of the nervous system. "Exposure for a single hour at night," says Mitchell,² sometimes produces an almost immediate attack, sometimes causes a *tendency*, not expressed perhaps for some months." Mungo Park, alluding to African experience, says: "The rain had not continued three minutes before many of the soldiers were affected with vomiting, others fell asleep, and seemed as if intoxicated. *Twelve of the soldiers were ill the next day.* He felt the same symptoms himself. The following cases, occurring lately at Whitworth Hospital, reported in the *Irish Hosp. Gaz.* (*Boston Med. and Surg. Jour.*, Nov., 1874), afford an additional illustration:—Two hospital nurses, accustomed to hospital sights, odors, and sounds, were subjected

¹ Virchow has lately said: "Fever consists essentially in elevation of temperature, which must arise in increased tissue-change, and have its immediate cause in *alterations of the nervous system.*"

² "Essays," edited by S. Weir Mitchell.

to a most remarkably fetid odor from the alvine dejection of a dead body. Within ten minutes one fell down, vomited, and became unconscious. After several remedies, an ice-bag to the nape restored consciousness in a short time. The other nurse vomited while at the dead woman's bedside, went to her room, and fell down in an unconscious state. Half an hour after the ice-bag was applied to the spine she regained consciousness. Dr. McDowell was of opinion that the medulla oblongata was under the toxic influence of the noxious gases.

As explanatory of the manner in which the fever-poison may thus suddenly influence the system, I adduce a brief account of a very interesting experiment performed a few days ago by Dr. Horatio Wood, of Philadelphia, and of which he was kind enough to give me the particulars:—He divided the cord of a dog at the junction of the *medulla* with the *pons varolii*, and, of course, above the vaso-motor centres. Although the lateral sinus was accidentally divided, an accident difficult to avoid in so high a division, and “three-fourths of a pint of blood lost,” which should have *reduced* the temperature several degrees, there was a rapid rise to 104° F. (the normal temperature of the animal being about 102°). Here all the controlling influence on the peripheral nerves was lost, just as it is when a powerful toxic impression is made from without. *Cholera Asiatica* not unfrequently produces this sudden overwhelming effect on the nerve-centres, and patients drop suddenly.

Periodicity, or tendency to relapse at certain intervals, the most marked, by many considered the pathognomonic symptom of malaria, also indicates the nervous system as the seat of the disease. This and incubation, with which it is intimately connected, present, perhaps, the most interesting, the most extraordinary and inexplicable phenomena connected with the history of disease. That an exposure should not manifest its effects on the system for a week, or a month, or even many months, and should then, apparently without provocation, induce suddenly the most striking and even alarming symptoms: this phenomenon, this tendency to relapse, is often as annoying and perplexing to the physician as to the patient, and it is practically of the utmost importance that its mystery should be un-

ravelled. The very accuracy as to time points to the nervous system as that portion of the body to which we should direct our investigations; since, if we look *within* the body for the cause, as the slow accumulation or the growth of the *materies morbi* to the point of saturation and explosion, the blood itself, or any organ except the nerves, ought, it would seem, to give some indication of diseased action. And, on the other hand, if we look *without*, or for external impressions, "exciting causes," the very suddenness of the attack would seem to indicate that it must be through the medium of the same system.¹ Many cases might be cited in which none but *nervous* manifestations follow exposure to malarious influence—neuralgia, especially supra-orbital, for instance. A man, surrounded by malarious disease in all its forms, recently applied to me with a most atrocious pain over the left eye, coming on every other day at or near a particular hour. He could not credit my assertion that it was "fever and ague," as he had experienced none of the ordinary symptoms whatever. But a few full doses of quinine sent him to his work rejoicing. Periodicity, as has been already remarked, instead of indicating malarious fever, is rather a characteristic of nervous disease. Aitken adduces the periodicity of the phenomena of fever as evidence, not that the fever is of a particular *type*, but that the *nervous system* is influenced; also, the beneficial effect of quinine as corroborative evidence. We observe it markedly in epilepsy. Thus, in a case now under my charge, the paroxysms came on regularly every two weeks, on Thursday, even when the subject was at work, and feeling as well as usual. In another case the attack occurred every four weeks, at night, for a long period. In a third case, under the care of Prof. Hammond, which I saw to-day, and the particulars of which I obtained from the patient's mother, the fits, while the patient was under the influence of the amyl nitrite, taken three times a day, came on as follows:—Four days' interval, 3 fits; 4 days' interval, 1 fit; $4\frac{1}{2}$ days, 2 fits; $4\frac{1}{2}$ days, 2 fits; 4 days, 1 fit; $2\frac{1}{2}$ days, 1 fit.

¹ There is sometimes a marked periodicity in *chorea*, and Dr. Weir Mitchell, who is eliciting some curious and interesting phenomena in his investigations concerning this disease, finds that the attacks *usually* recur in the spring.

The symptoms following the ingestion of poisons sometimes show marked periodicity. Christison, as quoted by J. K. Mitchell, informs us "that a whole family, consisting of a woman and four children, were attacked by a tertian fever by living for four months on *edible mushrooms*." The husband, who lived on other fare, escaped the symptoms. "Westerhoff observed, in those who were poisoned by mouldy food, an *intermittent somnolency*. M. Gassand saw cases of *ergotism*, where the sensations, either of heat or cold, were *intermittent*." Other writers mention this feature. "The *mental disturbance* intermitted in one case, *inflamed eyes* in another, and all the phenomena in a third." "The *Dic. des Sci. Méd.* reports cases of this kind, in which occurred the *most acute pains* which were intermittent." In these cases it was also noted that the preparations of *bark* were the most effectual remedies. Dr. Mitchell adduces these facts, among hosts of others, in support of his fungous theory of "malaria." Whether he is right or not, they strongly support the position I am endeavoring to sustain. His essay will well repay perusal. But the most striking instance of marked periodicity occurring from a purely nervous disturbance, happened in a case published by me in the *Amer. Jour. Med. Sci.* for July, 1862. A girl, eleven years of age, and in fair health, not having been subjected to any malarious influence whatever, but of an exceedingly nervous temperament, received a wound of the supra-orbital region, from a fragment of copper-cap, producing amaurosis. The removal of the foreign body almost immediately relieved the defect of vision; but, on the second night after, the eye became much inflamed, deep-seated pain set in, nausea, coldness, and "numbness" of the extremities, and great nervous excitement. Pulse and skin natural; no fever. Anodynes were rejected by the stomach, and the pain was much mitigated by tinct. aconite locally applied. Next morning was pretty comfortable. This was on the 8th of July. On the 10th, another severe attack supervened; the pain was particularly severe in the back of the neck, with inability to flex the neck forward. Skin hot, pulse frequent; vomited. Ordered mild antispasmodics. Fell asleep after one dose, and slept quietly. In the morning quite com-

fortable; skin pleasant; pulse natural. On the 12th, had a similar attack, except that the skin and pulse were but slightly affected. One dose put her to sleep. She was now ordered a grain of quinine, as a tonic, three times a day. Her next attack, on the 14th, was similar but very slight. She had been ordered five grains quinine on that day as a prophylactic. On the 16th had a violent attack of pain in the eye, but no other symptom. Quinine had been omitted by mistake. The eye looks much worse, and vision is much impaired. July 30th: Vision perfect. Dec. 14th: Patient has continued well, but has *visus dimidiatus* of the affected eye.

Robert Jackson, who wrote on this subject three quarters of a century ago, seems to have had a clearer idea of the causes and effects of malaria than any writer of more modern times; and had his knowledge of the anatomy and physiology of the nervous system been equal to that of the present day, might have solved some of the intricate problems which have perplexed us ever since. All his theories, his experiences, and his observations seem to converge towards the nervous system as the *fons et origo* of the diseased action, and to the necessity of sustaining this, and protecting it from morbid influences, not by any special remedy, but by a combination of judicious management, hygienic and therapeutical, moral and mental, as well as physical. Were it not for fear of rendering myself tedious, I would like to quote a page or two from his work, modestly entitled, "Remarks on the Constitution of the Medical Department of the British Army," published in 1803, which may be found in the library of the New York Hospital.

It may seem to be diverging from our line of investigation to cite *articular rheumatism* as an example of a neurosis. But I ask your attention, for a few minutes, to some well-known facts in its history and treatment, and also to a remarkable memoir of Dr. J. K. Mitchell, to be found in the volume of essays to which allusion has already been made. He attempts to prove rheumatism a neurosis, and refers its phenomena all to a spinal origin. His cases show a striking resemblance, in their articular symptoms, to acute rheumatism; and that these are caused by the spinal disease or injury, he

fully demonstrates. But they all seem to be cases of organic disease of the spine, or injury of the *medulla*, except one, which I abridge.

Robert Gordon, well known as the carrier of *Poulson's Daily Advertiser*, 56 years of age, vigorous constitution and active habits, was the subject of the following attack. Observing a severe pain in his right heel and ankle, immediately followed by redness, heat, and tumefaction, he caused himself to be largely bled, and took salts and magnesia. On the following day the symptoms increased and the ankle and knee of the opposite side becoming affected, he was confined to bed.

On the third day, my first visit was made. Had then a full, strong, frequent pulse, flushed face, dry skin, whitened tongue, and complained much of the severity of the pain in his legs, and his incapacity to endure the slightest pressure or motion. I directed the application of seventeen cups to the lumbar region, so as to abstract twelve or sixteen ounces of blood. Next morning, I found the pain almost entirely gone; does not complain of moderate pressure, and is able to move his legs without inconvenience. Ordered a draught of salts and magnesia, and a lotion. Third day: Pain in legs scarcely perceptible, but the shoulders, elbows, and wrists are beginning to exhibit marks of severe inflammation expressed by pain, tumefaction, heat, and redness. Ordered twelve cups to the cervical spine. Fourth day: Patient sits up; complains of stiffness, but no pain except in one wrist, and that very slight. Epsom salts and magnesia. Fifth day: Finding nothing for which to prescribe, arranged patient's diet, recommended occasional use of aperients, and took leave of the case.

Called on tenth day to inquire into the results, and found there had been no return of the disease. Since that time a severe winter has passed, and he has continued in a good state of health.

The reader will perceive, says the reporter, that the general bleeding, though very copious, proved of no service, and that the large local bleeding in the lumbar region benefited solely that part of the disease which lay at the peripheral extremities of the nerves supplied by the lower end of the

spinal marrow. The inflammation in the upper extremities continued afterwards in progress, and was arrested only when cups were applied over the cervical spine. In this connection I refer you to an article on "Neuroses of the Joints," in the Sept. No. of THE PSYCH. AND MED.-LEG. JOUR., translated from the German of Moritz Meyer. In concluding his essay on rheumatism, Dr. Mitchell adverts to "the close connection between rheumatism and certain diseases of the mucous and fibrous tissues of the eyes, nose, mouth, alimentary canal, bladder, and urethra. In many cases, diarrhoea and dysentery are found to alternate with rheumatism of the extremities, and particularly of the lower limbs; wherever such cases happen, they are always found to yield more readily to spinal treatment than to any other mode of cure, thus affording another proof of the spinal origin of these cases." In a case of severe pruritus, reported in the first number of the *Archives of Dermatology*, by Dr. Beard, for Dr. Kinsman of Ohio, the latter remarks: "The moment the current (of electricity) is passed along the spine, the pruritus ceases." Dr. Beard also reports a case of herpes zoster frontalis for Dr. Bulkley, cured by galvanism. I have thus also promptly relieved cases of herpes zoster. The spinal origin of many cutaneous affections is now well established, therefore such illustrations need not to be multiplied. With reference, however, to the last named disease, I desire to refer briefly to some remarkable evidences of its purely neurotic character, as well as of other vesicular affections, contained in M. Vulpian's recent French edit. of S. Weir Mitchell's "Lesions of the Nerves," and hence, to the very great importance, in all cases of very obstinate eczematous or bullar diseases, of paying special attention to the influence of the nerve centres, and of possible nerve lesions, as a cause. One case is that in which a ball, traversing the chest, without injury to the skin, except at points of entrance and exit, induced a very painful herpetic eruption. Brown-Séquard mentions a confusion of the brachial nerve causing a herpetic eruption all along its course. Sometimes the eruption is analogous to that of pemphigus. M. Raynaud cites a case in which an injury of the trunk of the

"cubital nerve" was followed by an eruption along the whole course of the nerve. "The list of such facts," says Mitchell, "may be extended as far as one may wish." He says that his term "eczematous" to such affections has been sharply criticized by Chareot and Handfield Jones. How little should we regard the trifling eruption, sometimes only lasting a few days, which marks the onset of *herpes zoster*, when the neuralgic symptoms are so much more pronounced and so difficult of cure; sometimes, indeed, under the old methods of treatment, lasting through the remainder of life. Electricity now holds out a prospect of a speedy cure. In fact this disease is as much out of place in a work on *skin* diseases, as a whale would be in a treatise on fishes.

The very great uncertainty attending the classical treatment of rheumatism through the blood, and the effects of other treatment acting in a totally different manner, do not give much support to the theory of blood-poison. Quinine I have found to act sometimes with great promptitude in aborting the disease when acute, even when there is no evidence of malarious poison. Some rely upon it as a routine treatment. M. Sée, of La Charité, remarks in a lecture recently delivered there, that he considers it of great value in acute rheumatism, and always returns to it with benefit after a trial of all other remedies. He says, "its effect is on the spinal cord, in lowering its irritability, and thus diminishing the sensibility to pain, and lowering reflex excitability, thus reducing the afflux of blood to the inflamed joints." "It may be mentioned," says the *Lancet*, August 8, 1874, "that this mode of treatment is adopted by a large number of the leading physicians of Paris, either alone or with other means, and they all appear to be unanimous in its favor." But to be successful, the drug must be administered with no niggard hand. The blister, or counter-irritant treatment as it is sometimes called, is not seldom promptly successful, if carried out with the firmness which only will insure success. The blisters require to be large, and to be applied freely, and rarely give any annoyance. Patients often beg for their repetition. Ice to the joints is a favorite and success-

ful remedy with some. Esmarch and others used it with great success in the Franco-German war. Dr. Da Costa has recently used the bromides successfully.

The application of cold to the whole surface of the body, or cold bathing, packing or affusion, in cases of extreme danger both in rheumatism and *fevers*, is a still more potent method of treatment, and affords, perhaps, a still stronger evidence of the neurotic character of these diseases. When the temperature, in these cases, runs up to 109° or 110°, though it may go still higher, it was found that death was the almost inevitable result, until after the adoption of a certain bold treatment, which consists in a rapid reduction of the abnormal heat by the application of cold to the whole surface.¹ It appears to be generally understood that the good effect is due to the direct abstraction of heat. But it is doubtful if this has much to do with it, if it has anything at all. Frequently the internal temperature continues to fall for some time after the patient has been put into bed and covered up, to such an extent as to threaten serious consequences at times. Now, the mere abstraction of heat by direct action of the cold must cease the moment the cold is withdrawn from the surface, and we must look for some explanation of the continued fall of temperature. Then there are cases in the so-called hysterical conditions of females, where the surface is habitually cold and clammy, and yet the best remedy is the cold shower-bath. I am happy to be able to adduce the experience of so able and careful an observer as Robert Jackson, who speaks, as regards fevers, from an experience of the largest kind. "It is possible," he says (*opus cit.*), "that excess of heat may exist, and it actually does exist without *superficial excitability*, that is, without a due share of sensibility of surface (*italics mine*) both in the early period and in the later stages of fever. Such condition of fever is common in spring, common in Europeans soon after their arrival in tropical climates, both in the commencement and in the after-period of the disease, either as connected with plethora, or with internal congestion. The heat is then often

¹ The treatment is not new as regards fevers, but was proposed and largely practised by Currie, but fell into comparative disuse.

ardent—particularly on the trunk of the body. A thermometer, in this case, is a fallacious guide. It indicates a high temperature, but experience proves that cold bathing does no good; it probably does harm in the cases connected with internal congestion. To trials, in such cases, it is believed the credit of the remedy has been sacrificed in the fever of the West Indies, and probably in that of America.” That is to say, in modern phraseology, the cutaneous nerves must be in such a state as to receive and convey an impression to the nervous centres, thence to be transmitted to the vaso-motor nerves of the internal organs, contracting their minute vessels, diminishing hyperemia, arresting, for the time at least, destructive metamorphosis of tissue, and over-production of animal heat.¹

As a remarkable illustration of the powerful and sometimes destructive effect of applications to the surface, when too extensive, too severe, or too prolonged, I remind you of the results which followed the old experiment of varnishing animals, and the death of the child coated with gold leaf, to represent the golden age in a procession during the reign of Louis XIV. The cause of death in these cases has been variously attributed to the great fall of temperature which ensues, to the complete arrest of perspiration, to asphyxia, etc. But lately, Dr. Feinberg has been reinvestigating the subject by further experiments, and has come to the conclusion that it is “from paralysis of the *vaso-motor nerves* all over the body, producing excessive dilatation of the vessels, and extensive extravasations. In the spinal cord, especially, was there congestion and extravasation.” There was decided sinking of the temperature. He attributes the vaso-motor effect to intense irritation of the sensory nerves of the skin. His specimens were examined by the celebrated histologist, Frey, and his facts confirmed by him.

The coincidence of *excessive dilatation* of the capillaries of the viscera, with marked *reduction* of temperature, may seem

¹ It is not contended that this is the only mode by which the impression may act on the nervous centres. In fact, the *inhibitory* action developed in those centres, as will be shown by examples, may be still more powerful, even to the production of fatal consequences.

to conflict with the views frequently expressed or implied in this paper, as to the increase or diminution of temperature from vasal dilatation or contraction respectively, though we have been careful not to attribute it exclusively to this. But we must not forget that, in Feinberg's experiments, this vaso-motor paralysis is carried to an *extreme* degree. In like manner, though the physiological effect of some of the drugs, to which your attention will presently be asked, is to *contract* the vessels, they are found, if pushed to an extreme or fatal degree, to do just the opposite; and this ought always to be borne in mind in estimating the peculiar properties and therapeutic value of these powerful agents. So here, the immense reduction of the intra-vascular pressure, producing sinking of the heart's action, and almost complete arrest of the circulation in the capillaries, would naturally induce a fall of temperature; whereas the varnishing of a smaller portion of the surface would have doubtless produced quite a different condition of the visceral vessels.

To return to a consideration of the *joint* affections of rheumatism, I desire to remind you of the many instances where pain and inflammation of the larger joints supervene on injuries, and suppurative diseases of the extremities, and on other conditions giving rise to what is usually called septicæmia or pyæmia, supposed to be due to blood-poisoning, or to a low grade of inflammation extending along the veins and absorbents. A patient of mine, a lady eighty-three years old, recently died of senile gangrene of the right foot, advancing slowly at first, but finally running up the leg with great rapidity, attended by no chill or febrile reaction, but affecting the sensorium profoundly; simultaneously was developed a "rheumatic" inflammation of both wrists, and for some days the only pain which she appeared to suffer was due to this. Here there was no evidence whatever of blood-poisoning. In view of this, and many similar facts, it would be well to review our ideas of the causation of these intercurrent phenomena, and to remember the connection which may exist between what has been said about neuralgia and the spinal origin of rheumatism, and the fact that in the cases just mentioned, those of supposed septi-

cemia, quinine is now considered the sheet-anchor. I am told that Sir William Gull, twenty years ago, claimed a spinal origin for rheumatism, and thought the idea original, but was preceded a quarter of a century by our distinguished countryman.

We are, of course, not able to experiment on the human body as we do on animals, in order to solve physiological problems, but accident frequently comes to our aid; and the evils of war are often turned to good account. I desire to call your attention to a few instances of this kind which serve to elucidate the influence of the nervous system in the production and cure of disease.

The following case fell under my notice while on a visit to Saratoga Springs, in the autumn of this year, and, I think, demonstrates that which had only been previously done by experiments on animals, what Austin Flint, in his *Physiology*, calls the starting-point of our definite knowledge of the functions of the sympathetic nervous system—"that the influence of the sympathetic nerve in the neck was propagated from below upward toward the head, and not from the brain downward." This was carried further by Brown-Séquard in 1852, whose experiments led to the discovery of the vaso-motor nerves.

Case.—Theophilus S., stair-builder, fell on June 22d, 1874, twenty-five feet; the spinal column, at the base of the neck, and from that to the right shoulder, striking a hand-rail. The blow was thus received rather on the right side of the spine than directly upon it. Had always been well, and had never been injured until the Franco-German war, when he was stunned while in charge of an ambulance by the explosion of a shell. He soon recovered, and is not aware that any ill consequences followed. Last winter he was greatly chilled by a somewhat prolonged exposure, since which he has not been quite so well as usual. After his fall, he was unconscious for an hour. Dr. Whiting saw him at once. There was no indication of any injury to the head. On returning to consciousness he complained of a most intense pain in the occipital region, low down; the least movement of his body gave him intense pain at *seat of injury*, and nowhere else. He also felt as if the whole right

side, the arm, leg, etc., "were dead." No uneasy sensation in left side. His vision was also impaired, and he had "noises in his ears." He required a hypodermic dose of morphine for his headache. Was constipated for several days, and had retention of urine, lasting three days, and then passed his water, and continued to do so. For five days could not move his right lower extremity at all. Could move his fingers, but any attempt at movement of the arm produced violent pain in the spine. Motion and sensation in the limbs returned very gradually. For a while the headache required anodynes, and gradually subsided.

Five weeks after the accident he felt so much better that, with the aid of a stick and limping, he walked about a quarter of a mile to the doctor's office. A day or two after, he was much worse; was put to bed again. Severe gastric and abdominal symptoms set in. After eating one or two mouthfuls of food, there was so sudden and enormous an accumulation of gas that he felt as if he would burst; had other symptoms of indigestion, and an obstinate diarrhoea set in. Headache also returned and required the hypodermic injections, which acted very slowly. About eight weeks after the first relapse he felt so much improved that he went to work; worked only two hours and a half, and with great difficulty. Pain commenced in his shoulder and back, then his fingers stiffened so that he could not hold his hammer; headache set in and his old diarrhoea; he could scarcely get home on account of the latter, stopping three times to relieve himself. He was better after a rest in bed for some days. It was soon after this, when he was able to walk about a little, that he came under my notice in Dr. Whiting's office. His prominent symptoms were as follows: vision imperfect; since his first relapse sees everything sometimes blue, sometimes red; and, on lying down, sees things "quivering," and in this position his headache, now very moderate, is decidedly increased; sleeps badly, was somewhat relieved by moderate doses of bromide of potassium. Sleeps pretty well when *sitting* in an easy-chair, and better in the daytime. His diarrhoea is uncontrollable by opiates and astringents, except for a short time. When his bowels are loose,

his micturition is painful. The *sudden* puffing up of his stomach, on taking a few morsels of food, was a marked and intractable symptom for some time, but he is now, in a great measure, relieved. Another very curious symptom has lately appeared; his evacuations are attended by an intense burning sensation in the bowels and around the anus, "*as if sprinkled with red pepper.*"

I have not the space to comment on this remarkable case. I showed him to Dr. Hammond, and Dr. Cross also saw him. They could not make up their minds that all his cerebral symptoms were due to the spinal injury. Perhaps they were not. But the abdominal symptoms certainly were; and, in connection with this, and with the remarks on the spinal origin of rheumatism, and the nervous origin of fever, I adduce other cases of abdominal disease caused by and causing affections of the nervous system, and relieved by remedies addressed to that system.¹

A remarkable case of convulsions and coma, caused by a slight abdominal irritation, is related in a letter from Paris, from the parent of the patient, and published in the *Boston Med. and Surg. Jour.*, for Oct. 29, 1874: "The child while at dinner, and in perfect health, was suddenly seized with a convulsion of a most violent and alarming nature, becoming rigid, eyes fixed, pupils much dilated, teeth clinched, accompanied by the most distressing gasping for breath, and complete unconsciousness. The physicians summoned tried the warm bath, artificial respiration, etc.; the *hot* bath, dashing cold water. One proposed a hypodermic injection of morphia, which seemed to relieve the violence of the spasms. Sir John Rose Cormack arriving, suggested another hypodermic injection, and, in case of failure, *tracheotomy*. The injection fortunately succeeded; he became narcotized, after having been in violent convulsions for two hours. The diagnosis was *worms*. The following

¹ See case, very similar, in many respects, to that of Shires, in *London Practitioner* for March, 1874, extracted from *Deutsch Archiv für Klin. Med.* 1873, xi. The very first application of the galvanic current to the left cervical sympathetic relieved the violent cerebral symptoms materially.

day he awoke quite relieved. He subsequently passed two or three worms.¹

I had a case very similar to this, in a child nine years old; the cœna was complete for some hours, and death seemed impending. I also diagnosed correctly, as the event proved. There is really nothing remarkable about these cases, except as regards the age. Scores of *infants* in our large cities die every summer from convulsions induced by abdominal irritation and reflex action, many of whom might be saved by a better appreciation of neuro-pathology, but especially by a more liberal use of neurotics, and by bolder doses of *opium*, with the temporary aid afforded by chloroform inhalation. Reflex meningeal irritation is still constantly mistaken for inflammation, and the traditional horror of opium in meningeal and cerebral diseases of infants still stays the hand of the physician, and consigns many a curable patient to an early and untimely grave. In an article on the treatment of vomiting by electricity, published in the second number of Beard's *Archives of Neurology and Electrology*, will be found three cases there related by me, remarkably illustrating the nervous, or, if you choose, malarious origin of diarrhœa, and its prompt arrest by electricity, after total failure of all other ordinary remedies—the cases of Mrs. L., Mrs. M. C., and the girl A. D. In the first, however, the extract of ergot, given for a different purpose in large doses, by hypodermic injection, was largely instrumental in the cure. And here I will supply an important omission in the case of Theophilus S. I suggested to Dr. Whiting the use of ergot and bromide of potassium for the diarrhœa; and doses of half a drachm (fl. ex.) increased to a drachm of the former, with half-drachm doses of the latter, relieved him completely in one week. The fluid extract of *Eucalyptus Globulus* (which, however, differs from ergot in possessing astringent properties) has

¹ See a remarkably interesting case of most serious cerebral symptoms, caused by constipation, in *Brit. Med. Jour.*, August 8, 1874: *Med. News and Lib.*, Oct., 1874. Since the above account was written, I have met with a full and extremely interesting account of its history, in the *London Medical Times and Gazette*, from the pen of Sir John himself. This I propose to notice more fully in connection with a case treated by me within the past week.

a similar effect to ergot in these cases, an effect not due to its astringent action, as proved by the previous failure of other more active remedies of this class. A combination of the two is excellent; and the powerful tonic property of the *cuculyp-tus* aids essentially. Prof. Hammond has found this combination admirable in a similar condition of the mucous membrane of the bladder, arising from an atonic condition of its muscles, as well as the muscular coats of its vessels, and constituting catarrh of the bladder, with an alkaline condition of the urine.

Dr. Austin Flint, Jr., in his recent work on the "Physiology of the Nervous System," publishes the results of the experiments of Dr. Moreau, in Paris, in 1869, which are strikingly illustrative of the preceding suggestions and cases. "In those experiments the abdomen was opened in a fasting animal, and three loops of intestine, from four to eight inches in length, were isolated by two ligatures. All of the nerves passing to the middle loop were divided, taking care to avoid the blood-vessels. The intestines were then replaced, and the wound in the abdomen closed with sutures. The next day the animal was killed. The two loops, with the nerves intact, were found empty, as is normal in fasting animals, and the mucous membrane was dry; but the loop, with the nerves divided, was found filled with a clear, alkaline liquid, colorless, and slightly opaline." This experiment has an important bearing on the pathology of *cholera Asiatica*, the *profuse watery diarrhoea*, *without abdominal pain*, indicating mere want of *innervation*, rather than an attempt to *eliminate a poison*, and the *subsequent* cramps in the voluntary muscles, pointing very decidedly to the nervous system as that mainly, if not exclusively, affected by the poison, and to the necessity for a treatment which may present, at least, more uniformity of action than any which has heretofore been devised. For the history of this destructive disease, as far as *treatment* is concerned, is far from creditable to us as a profession; those methods, based on totally different pathological views, showing about an equal amount of mortality.

The peculiar effect of our *cathartics*, especially when they

are given for their so-called *derivative* action, can only be explained by their reflex influence on the brain, and its vasomotor vessels, rather than by unloading the *abdominal vessels*, which is also a means, but a more tardy and less efficient, since the impression felt in the brain is almost instantaneous. This is felt sometimes even after an ordinary evacuation, or even after the passage of gas. In the same manner a sharp pain in the cardiac region is instantly relieved by the sudden passage of gas from one portion of the intestines to another, the pressure on certain nerves being thus removed. All these apparently trifling phenomena, unimportant individually, are valuable when studied together.

Returning to the consideration of accidents and the influence of nervous shock in the production and cure of nervous affections, I merely refer to the two following cases published by me while house-surgeon to the New York Hospital. A negro with delirium tremens, upon whom I had vainly exhausted all my remedies, broke from his attendant and sprang from the third-story window, falling on the stone stoop below, and striking on one buttock. He was carried to his ward, was found to have sustained no damage save a severe bruise at the seat of injury, and in a state of perfect sanity. He was discharged cured in a few days, when able to walk. At about the same time Dr. Lyman Stone, then house-physician of Bellevue Hospital, published a similar case. The patient was wildly delirious from typhus fever, and the case concluded with this quaint sentence: "He broke from the orderly, jumped out of the third-story window to the area below, and had a rapid recovery." A case somewhat similar was related to me a few days ago, by an intelligent lady patient. A friend of hers, sojourning at Litchfield, Connecticut, during the past spring and summer, accidentally fractured her forearm. She was of a nervous temperament, and the shock was very considerable. The case did well, but the point is this: For forty years regularly she had had a recurrence of "hay-asthma" at a particular date; but this summer has passed with no symptom of it. The case is particularly interesting in view of Dr. Beard's recent researches into the *nervous origin* of this disease, and the means suggested for

its cure, of the efficacy of which he is very sanguine. See his paper recently read before the "Public Health Association." Now, I do not recommend or propose to employ such radical means of cure; but, in connection with the pathological questions involved in this essay, and the effects of the neurotics already alluded to, and those to be considered, they are suggestive.

In connection with the preceding reflections and hints—for I cannot call them arguments—obscure and uncertain though they may be at present, let us briefly review the physiological effects of some of the more important *remedies*, which, with the aid of a larger experience and a more extended series of experiments on living animals, may enable us to take advantage of what, it is hoped, may prove better views of pathology.

Among the more important of these presumed to act specially on the nervous system, may be included *atropia*, *ergot*, *strychnine*, *opium*, *conium*, *physostigma* or *eserine*, *iodine*, *calomel*, (in *sedative* doses), *quinine*, *phosphorus*, *amyl nitrite*, the *bromides*, the *anæsthetics* (by inhalation), *digitalis*, *electricity*. Many others might with propriety be added, but these have been most thoroughly studied as yet. *Guarana*, or *Paullinia sorbilis*, ought not to go unnoticed, though we know nothing of its precise physiological action; but the prompt relief which it affords in various headaches, including *migraine*, and the safety with which it may be administered in almost any dose, renders it, like the bromides, doubly valuable.

Electricity is placed last, not because it is the least in value, but because it is undergoing a thorough investigation, and its status is very unsettled. Judging from the power which it is rapidly developing over a great variety of diseases under the close study and thorough experimentation of many of the ablest men in the profession, it bids fair to stand high in the list of this class of remedies, and perhaps second to none in the *Materia Medica*. As an *analgesic*, and even *hypnotic*, it may, in a great variety of cases, aspire to a rivalry with opium—the *magnum domum Dei*—in some cases being far superior to it. If time will permit, I hope to adduce numerous cases in proof of this assertion, some of them very striking, a few of them I think *unique*, many of them not involving anything novel or strange

to those of you who are in the daily habit of employing this agent in its several forms largely in your practice. But, as yet, comparatively few even of those considered the best therapeutists of the profession, make any considerable or systematic use of electricity, except in a very contracted range of diseases, and are extremely skeptical as to the reports from specialists, of its wonderful influence over symptoms and diseases to which it had heretofore been considered entirely inapplicable. It is no wonder that the extraordinary assertions of Remak, Duchenne, and lately Arthius in Europe—to say nothing of their distinguished followers in our own country—should be received with discredit, and even with derision; it has invariably been the fate of all remarkable discoveries in our science. But an honest and careful imitation of their methods, uninfluenced by blinding prejudice, will soon convince the most skeptical that enthusiasm has but little colored or warped their reports; unless, indeed, the enthusiasm engendered by the almost magical results of his first essays should warp his own judgment. *Static* electricity has fallen into such complete disuse of late, and the little book lately published by Arthius is so peculiar and enthusiastic in its style, and so sneeringly alluded to by its reviewer in the last number of Hays' Journal, that I am induced to put on record here probably the first authenticated case of its curative effect in this country, and the first instance where it was used for *myalgia*, a disease for which we now know electricity is a specific. The operator was my colleague in the Council of the University of New York, the well-known divine Mancius Hutton, D.D., who will, I hope, pardon me for using his name in this connection. He was experimenting fifty years ago with a cylinder machine made by himself, when his father happened to be suffering from an obstinate attack of myalgia of the muscles of the shoulder and back; he placed him on an insulated stool, also made for the occasion, charged him with electricity from his machine, and then drew sparks from all the affected parts. He was promptly and permanently relieved.

The physiological action of *ergot* is now pretty well established. It contracts the minute vessels, both arterial and

venous. It also acts on the involuntary muscles themselves, as, for instance, the heart and uterus. It acts both on the vessels of the medulla spinalis and the brain and its membranes: this, and the fact of its being perfectly innocuous, even in very large doses, renders it, like the bromides, an extremely valuable medicine. The two together, and aided sometimes by belladonna, which has on the vessels a similar action, will probably prove one of our most reliable means of combating both acute and chronic affections of the encephalon. In spinal congestion Prof. Hammond's very large experience has taught him that, when perseveringly used in large doses, it is a most valuable resource. Much has lately been written about its influence on fibrous tumors, especially of the uterus. I can confirm the reports of its great value in these cases. The tumors, if large, rapidly diminish, and their more serious complications, as hemorrhage, are promptly relieved. Hypodermic injection is the most efficacious means. Many seem to have been deterred, after some trials, by the occurrence of inflammation, abscess, etc., and have tried suppositories of Squibb's solid extract, which succeeds, but not so well. Dr. Murdock and I have used the injections freely in one case (alluded to in my paper on vomiting, and on the hypodermic treatment of intermittent fever); 167 injections of Squibb's fluid extract were used, in many instances repeated in the same locality, after the resulting indurations had subsided, and only once with any untoward result. I attribute the effect on these fibroids to *starving them out*, by its constringing the vessels. This explains why it succeeds better on large tumors which are less nourished. When the body is poorly nourished, heterologous or adventitious growths are the first to be absorbed, and some of those tumors have been cured by the starvation system. Langenbeck thinks he has cured aneurisms of large arteries; indeed German surgeons have published several cases of cure, but the experiment does not seem to have been followed up, and it seems difficult to account for its effect on such large sacs or their contents. Lately it has been injected in the cellular tissue, over large varices, with a promptly curative effect; here its action is easily comprehended. Drasche, of Vienna, has lately

been credited with being the first to recommend and to use hypodermic injections of ergot for hæmoptysis and other hæmorrhages. But in October, 1869 (see report in *Medical Record* for November), and before the publication here of Langenbeck's trials, I used the undiluted fluid extract of ergot in a desperate case of post-partum hæmorrhage (20 to 30 minims), and the patient recovered. Its beneficial effects on certain forms of diarrhœa have already been alluded to, and its successful employment by one German and several French physicians in severe diarrhœa and dysentery, is alluded to by Wood in his recently-published treatise (op. cit.). It is well to combine it with *eucalyptus*, which, there is good reason to believe, acts similarly on the capillaries.

Dr. Crichton Browne, in the *London Practitioner* for June, 1871, and E. Churchill Fox, of the West Riding Asylum, in *West Riding Asylum Reports* for 1871, give clinical evidence of the excellent effects of ergot in mania. The latter makes the following important statement: "It has sometimes occurred to me that the bromide of potassium, the value of which I would be far from depreciating, has a tendency in some cases to aggravate the attacks of epileptic mania. It seems to relieve the muscular at the expense of the mental element in the epileptic condition. The fits are reduced in number and severity, but the paroxysms of mental disturbance are intensified and prolonged. Under such circumstances ergot becomes of the highest service; its use, alternated with that of the bromide of potassium, places the two phases of the malady under equal and powerful control." I have only space to say a word about the *dose* of this valuable drug. If it is used timidly or sparingly, or with a dread of the horrible consequences of *ergotism*, which has been too long the traditional raw-head and bloody-bones of routine practitioners and authors, it would be better to try some other drug. Less than *one drachm* of a good fluid extract (ergotine, so called, has no existence), three times a day, for an adult, ought to be the minimum dose, though we may *commence* with half a drachm merely out of respect for the stomach. In some cases of a very dangerous or obstinate character, the dose may be larger; and if for acute disease, or if

not to be continued very long, very much larger. "In one of Wright's experiments," says Wood, "an amount equal to two drachms for every pound weight of the dog, failed to kill." Horatio Wood says, "Although I have given ergot (fluid ext.) in ounce doses, I have never seen it cause any distinct symptoms." He alludes, however, to two cases, in which poisoning did occur; but the amount taken was not mentioned by the reporters. I cannot explain the statements of the numerous writers, who, for hundreds of years, have given accounts of fatal epidemics of ergotism, except by referring you to the testimony of Trousseau and Pidoux,¹ who "assert that these epidemics are not dependent on any specific action of ergot, but are either epidemics of blood diseases, or simply the results of improper and insufficient food—merely the outcomes of poverty, wretchedness, and famine." I have not referred to all the diseases for which this remedy has lately been successfully tried, but have devoted to it more space than I should have done, had not my friend Dr. J. C. Peters passed it over in his valuable paper of last month, on the vegetable neurotics.

I ask your attention next to *digitalis*, both because of its great value, and because it affords another still more striking instance of the evil of traditional evidence, or rather of a blind reliance on it. Up to a quite recent period in the history of medicine, it was looked upon as, *par excellence*, a depressing remedy, especially on the circulation; and I imagine that but comparatively few physicians now, taking the whole country, have any definite idea of the value of this remedy as a *heart tonic*, and of the great range of its applicability in disease. The error has arisen, probably, as errors concerning the action of remedies have too often arisen, from noting the effects of *toxic* doses on animals. In *such doses* "it lowers reflex activity, and induces lassitude, prostration, muscular tremblings, and sometimes convulsions." Its powerfully *tonic action* on the heart has been demonstrated by all the numerous experimenters on its action on animals. It produces such an energetic action on the heart, if pushed too far, "that the ventricles become white, as the last drop of blood is squeezed out of them." Finally, if pushed to

¹ Horatio Wood's *Mat. Med. and Therapeutics*, p. 469.

its poisonous effect, the heart is arrested in systole (Wood). In the case of a lady at West Point, a patient of Dr. Peaslee's, who had had a peculiar affection of the heart for some years, the pulsations being above a hundred, and very feeble, becoming extremely rapid, feeble, and irregular on walking across the floor, and who had been told, as she said, that there was no remedy for it, I prescribed tincture of digitalis in only ten-minim doses three times a day; within forty-eight hours, if my memory serves me, the pulsations of the heart and the throbbing of the vessels of the head and neck became alarming, so that in my absence she sent for Prof. Dunster, then the assistant surgeon of the post. He stopped the remedy, finding her pulse regular, full, and strong, and not abnormal in frequency. This was the end of her heart-trouble, for, I think, a year, when a similar attack, but much milder, was relieved by her physician at home, to whom she related the effect of the remedy in her previous attack. This is a typical, but not an uncommon case. It is not only in such marked cardiac cases that digitalis exhibits its great value as a neurotic. In all cases of debility requiring vegetable or mineral tonics, and when the heart's action is disproportionately weak, as is frequently the case, I employ it as an auxiliary, and with prompt and decided effect; also in combination with these remedies, or with astringents in passive hemorrhages, especially in some obstinate menorrhagias. It has been shown by direct experiments on frogs and rabbits, that it rigorously contracts the capillaries like ergot and belladonna (Boldt and Ackermann, quoted by H. Wood). Where an indication for the remedy seems to exist *from the weak or irregular action* of the heart, and a *concentric* hypertrophy of the latter is made out or suspected, the latter being usually considered a strong contraindication, it is well to give it, but to do so cautiously; and as the remedy is pretty uniform, and in my experience, and contrary to tradition, also *non-cumulative* in its action, it is perfectly safe; and I have seen it very efficacious in those very cases. In sudden cases of prostration from disease, or poisoning by such agents as paralyze the heart, as aconite or veratria, the remedy may be given by hypodermic injection. In Bellevue Hospital I am told it has been used thus

with prompt effect in sudden and dangerous suppression of urine, though its action on the kidneys is probably secondary to that on the heart and arteries, and not as a *diuretic*. It is well worth trying, commencing with ten minims. In severe cases of cardiac irregularity and frequent pulse, when the patient is not confined to the recumbent posture by necessity, it is better to enjoin it while the remedy is being tested, as it not only produces its effect by *direct action* on the muscular fibre, increasing its power, but by an inhibitory action on its nerves, allaying irritability and irregularity. It passes my comprehension how so advanced a man as Prof. Séé, of La Charité, can declare that digitalis is a cardiac depressor or paralyzer, and that Liebermeister and Headland should substantially sustain this opinion. This drug will be again alluded to under the head of paralysis of the heart.

Few, it is presumed, will be willing to admit *calomel* into the list of neurotics, or of remedies acting directly on the nervous system, and I can only refer you to actual clinical experience in my own practice and in that of others which seems to me to place it beyond doubt. I have not time to give recent cases, but I refer to numbers already published by me in the *New York Journal of Medicine*, March, 1856, in an article on Dysentery, and in the same journal for March, 1870, in a paper read before the Dutchess County Medical Society on the "Sedative Action of Calomel." As this peculiar and important action of calomel is scarcely at all comprehended at the present day, not even alluded to by Waring, Phillips, and Sidney Ringer in their late editions, though well known to James Johnson, Annesley, and many other distinguished topical writers of their period, as well as to Hamilton of Edinburgh; as it is entirely discredited by Headland, and not even referred to by H. Wood in his able and very valuable, though, in some respects, rather incomplete treatise so often referred to in this paper, I feel constrained to devote more space to its notice than I should otherwise have done.¹ "Probably no single dose," says Wood, is capable, in the average man, of acting as a virulent poison,

¹ Sir Thomas Watson says he has seen the good effect of the sedative doses, but he has apparently never used them.

owing to but a limited portion of it being absorbed, the remainder being swept out of the alimentary canal by the diarrhœa produced." If Dr. Wood would experiment with this, as he has so carefully done with so many other not so valuable drugs, and uninfluenced by tradition and prejudice, he would find that it neither passes out of the bowels quietly, "like so much chalk," as Headland says, nor is swept out by diarrhœa; for, when a *genuine sedative dose* is given to a sick person, for it is no test to give it to a healthy animal, as the Edinburgh Committee did, say not less than xx. to xxx. grains, it will, as I have shown by numerous published cases (op. cit.), sometimes produce constipation, not unfrequently a complete arrest of peristaltic action in dysentery and cholera for twelve hours; and even when it acts as a cathartic it does so not very promptly, and generally in the mildest manner; and ptyalism is almost never induced. It is true that if an overdose is given it does pass quietly out of the bowels without injury, as Headland says, but he is in error when he says that only the *ordinary* cathartic dose takes effect, say 5 or 10 grains. For if in the cases above alluded to such a dose be given, it will usually, probably always, produce irritative and aggravating effects, whereas the full dose acts in a manner precisely the reverse. There is, probably, as Wood states, no such thing as a poisonous dose of the drug, but I have never had occasion to go further than 90 grains in one night to a child with membranous croup, and I have always seen either good effects or no effect at all from such quantity. I refer to an interesting case of Hamilton's, in the person of a child of the celebrated Sidney Smith, published in his biography. The country practitioner, frightened at the doses recommended by Hamilton, abandoned the case, and the father carried out the treatment and saved his son.

That calomel has its sedative influence by direct action on the peripheral nerves, and so on the nerve-centres, and that, when more is administered than is required for this effect, the surplus is not absorbed, and does not affect the blood, is not only sufficiently proved, as far as clinical evidence can prove it, by the large number of recorded cases of my own and of many other observers in this city, but especially in the Western and South-

western States, but has remarkable corroborative evidence in the similar action of a remedy taken from the *végétale* kingdom. Although one drop of croton-oil is a dose, and two or three, which are very rarely given, will often produce alarming diarrhoea and prostration, a drachm or more has been accidentally swallowed without serious results. "Cowan," says Wood (op. cit.) "has recorded the case of a child, four years old, who recovered in two days, from a teaspoonful of croton-oil taken on a full stomach." Adams, another case of recovery from a drachm taken by injection. Wood himself has only been able to discover two fatal cases from large doses. In one, the dose was two and a half drachms, and the patient a man already worn out by a four weeks' illness from typhoid fever. In the other, recorded by Giacomini (Stillé's *Therap.*), "twenty-four grains" proved fatal, not by purgation, there having been only four passages, but apparently by nervous shock, the symptoms being those of "general collapse, the patient preserving consciousness to the last." When injected into the veins, the weight of evidence from experimenters on animals is to the effect that it does *not* thus produce its characteristic effects (Hertwig and Bucheim). In certain conditions of the peripheral nerves of the intestines, as where they are poisoned by lead (Colica Pictonum), and obstinate constipation results, the ordinary dose of the oil has no effect, I have repeated the dose, one or two drops, every hour, until ten or fifteen drops were taken, without eliciting any response, and with no evidence of absorption into the blood. This peculiar action of lead on the intestinal nerves renders its preparations a valuable remedy in some forms of diarrhoea, but the doses must be large enough, and they are seldom so, to produce this sedative effect. The fatal dose, in the latest work on Mat. Med., is one to two ounces (H. Wood). The danger of affecting the system seriously, is in giving small doses continuously—the almost infinitesimal doses which painters, plumbers, etc., sometimes get, producing all the serious phenomena of chronic lead-poisoning. So it is with calomel; there is absolutely no danger from a teaspoonful, while a few grains, given in minute doses, may produce the most serious consequences—in the one case, the medicine acting through

the *nerves*, in the other, through the blood. This peculiar immunity from large doses of drugs, while small or moderate doses produce such serious consequences, deserves more investigation than it has received. Take *arsenic* for instance, reputed one of our most active poisons: while unpleasant consequences arise from fractions of a grain, and death, it is said, from two or three grains, there are instances on record where several drachms, in one case two ounces, were taken, and the patients recovered; in one case a very large quantity was taken in lumps, and sixteen hours elapsed before remedies were applied. It has been said that these large doses cause prompt emesis, and thus a large portion is rejected; and, in such cases as the last, that it was in a very insoluble form. But I have, within a few days, seen a remarkable case where none of these theories could be applied. I was invited by Dr. D. I. Bruner, of Columbia, Pa., to see the patient, and from the two I obtained a complete history.

Mr. W. B., aged 55, a healthy man, but rather addicted to the favorite German drink of that region, had been meditating suicide, as he informed me, for three weeks; when, on the 19th Sept., 1874, having warmed up his courage by some five or six glasses of lager, he obtained an ounce of arsenious acid, in powder, from the principal druggist, under a specious pretext, and took full three-fourths of it, six drachms. He had just before taken some food. Supposing that death would come soon, he laid himself down in a proper position, informed his family of what he had done, and bade them good-by. His wife summoned Dr. B. at once. The doctor found him comfortable, but endeavored to persuade him to take an emetic, but he positively refused to have anything done. The doctor remained with him an hour and a half, and then left, supposing there must be some deception; an hour after, however, he was again hurriedly called, and found the man in great agony, vomiting, purging, and complaining bitterly of the usual pains and burning sensations in stomach and bowels. He had taken the ipecac left by Dr. B. It was not until *five hours* after the ingestion of the poison that Dr. B. got the usual antidote, which he gave him in large quantity as fast as he rejected it. None

of the arsenic was observed in the vomited matters; but the doctor did not see the first. He gradually recovered from the pains, and fourteen hours after the dose was taken he fell into a quiet sleep, with a natural pulse, easy respiration, normal temperature (by the thermometer), and rested four hours, and awoke free from pain or burning, and has had none since. In fact, with the exception of the first few hours, he has had not one of the many characteristic symptoms of acute arsenic poisoning, and a hypodermic injection of morphine would, in all probability, have promptly relieved those from which he did suffer. The nausea and vomiting continued about four days, but during that time he took and relished all the food he was allowed to have, and his appetite and digestion have been unusually good up to the present time, seven or eight weeks. I said there were no characteristic phenomena of acute poisoning. He did have marked injection of the conjunctivæ, and a slight feeling of constriction of the pharynx for a few days. On the 7th day, symptoms of chronic poisoning set in, with numbness and pricking, closely followed by weakness of the feet first, then the hands, and in the course of a week increased to the degree in which they exist now. Nov. 12th, he can close his fingers on my hand, but cannot grasp. He cannot stir foot or toes; can use the muscles of the thigh. The feet are very slightly œdematous, but only from letting them hang down most of the day. The functions of all his viscera appear to be perfect; his tongue is clean. His pulse, sitting up, is 120, and rather feeble. Recumbent, pulse 90. He can feel pricking or pinching in almost all parts of the foot, and refer it to its proper locality, but the sensation is always that of burning. Has been on the iodide of potassium. Within the past week the doctor thinks he has improved a little. Recommended increase of the dose (10 grs.) of the iodide, strychnia hypodermically, and electricity.

Bromide of potassium sometimes acts in a similar manner as regards dose; some physicians have given it in half-ounce doses, with good results. One physician knew of a case in which an ounce was taken at a dose with no unpleasant result; and a medical acquaintance, whose name I cannot now recall,

had a patient, during the war, to whom an orderly gave a saturated solution, containing *two ounces*, for dispensary use, without its being known until the following morning. The effect was not greater than it sometimes is from half a drachm. A lady patient of mine took nearly half an ounce, in a state of desperation from insomnia, and showed no other bad effect than want of co-ordination of the muscles for some days. She was rather benefited otherwise. One unpleasant effect, however, of a concentrated solution, and of even the little lenticulars of Dunton, on an empty stomach, is a most violent though transient pain in the epigastrium.

The above phenomena have also been noted in the case of *quinine*. A case has recently been reported to me by Dr. Woodhull, a former surgeon of volunteers, where one of his men took an ounce of quinine, and no marked symptoms resulted. He sometimes gave half-ounce doses, and frequently doses amounting to half an ounce daily.¹ H. Wood, however, refers to the case of a man and wife, the former of whom took two ounces, and the latter one ounce, at one dose. The man died, and the wife "was recovered with difficulty." Many physicians believe that, after reaching a certain dose of the drug, say fifteen to thirty grains, you cease to get any response, and the larger doses are simply waste of a valuable drug. I am myself inclined to take this view, as in the case of calomel, half a drachm of which, at one dose, is as far, probably, as it is ever necessary to go.

¹ New York Med. Journal, Art. "On Hypodermic Treatment of Fever," by the Author.

Pages 33-77 missing

THE NEUROTIC ORIGIN OF DISEASE, AND THE ACTION OF REMEDIES ON THE NERVOUS SYSTEM.

BY FREDERIC D. LENTE, M.D.,

MEMBER OF THE COUNCIL OF THE NEW YORK NEUROLOGICAL SOCIETY.

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(Continued from January number.)

Quinine has already been discussed in this paper in some of its relations, especially as regards its action on malarious and neuralgic affections. Its power of arresting or retarding the amoeboid movements and migration of the white corpuscles of the blood, and thus preventing inflammation or suppuration, asserted by Binz, has been lately called in question by other observers, and his inferences, rather than his facts, discredited. He also claims that, as quinine arrests putrefaction and fermentation by killing the spores or fungi which cause these changes, it will also arrest or relieve septicæmia. But his experiments on animals do not sustain this view. It is generally admitted, however, that large doses of quinine do have a remarkably beneficial influence on septic diseases; and we can only explain it by its action on the nervous system—such an action as we have claimed for it in other affections. The antipyretic virtue of quinine, so highly commended of late, has also been denied, but principally, I think, in consequence of observations made on *healthy* men, which is a most fallacious guide, except where ocular inspection of the physiological processes can be had. Dr. H. Wood, who ably discusses this question of the various actions of quinine on the system, comes to the conclusion that “it exerts in febrile disease a decided but feeble antipyretic action.” He, however, seems to draw his conclusions almost entirely from German sources. The experience of American physicians will be found more favorable. There is nothing, in

all the various and ingenious experiments and observations, it will be noticed, which warrants the generally received opinion that it has any modifying influence on the blood itself, or any antidotal effect against supposed disease germs, either vegetable or animal. But on the blood-vessels its action is not so well settled. This is an important question, and has attracted the attention of our sister Society at a previous meeting, but the experiments and observations which had then been made¹ were too meagre to give much support to either side. The result of Dr. Hammond's experiment with his *cephalohamometer*, was opposed to that of Dr. Janeway, who used ocular inspection of the vessels, through a similar opening in the skull. It is a little singular that, when Binz and others in Germany, were observing in so many instances, the effect of quinine on the *contents* of the vessels, they should have made no note on the condition of the vessels themselves. But I am able now to supply to a certain extent this oversight.

I learned from Dr. George R. Cutter, one of the surgical staff of the New York Eye and Ear Infirmary, that he had occasion to examine into this matter in Germany, and he was kind enough to give me the results from memory, as he had lost his notes. I extract the following interesting account from his letter:

"While working in Prof. Julius Arnold's laboratory in Heidelberg, during the winter of 1870-71, a Russian, whose name I have forgotten, was making a study of the effect of quinine on the system. At his request I examined the dogs, rabbits and frogs with the ophthalmoscope, and made measurements of the retinal vessels, before giving quinine, as well as at various periods during the administration and afterwards. A gradual but marked diminution of the *arteries* took place, and before the death of the animals, the arteries became mere threads. The veins were also much diminished in diameter.

¹ If the effects of these drugs were produced through the medium of absorption into the blood, we could hardly reconcile such a theory with the above remarkable facts. It would, however, seem that a fluid preparation, like croton-oil, lying for hours in the intestines, must be absorbed to a great extent; but, supposing this to be so, it does not produce its characteristic effect, as we have seen by the result of injection into the veins.

The animals often became blind, and the retina was anæmic to the highest degree. The mesenteries of frogs were exposed under the influence of woorara and without. Quinine was injected into some of the frogs. The mesenteries were kept warm, and covered with serum. Under the microscope the effect of the quinine on the arteries was well marked. Inflammation was delayed. The *answandering* of the corpuscles was also very much retarded and diminished, and the inflammatory dilatation was lessened to a considerable degree.

These results were also *controlled* by Profs. O. Becker and J. Arnold, Jr.¹

The effect on the corpuscles, mentioned by Dr. Cutter, agrees with the observation of Binz and others, to which I have just alluded.

Strychnine is one of the most important of the neurotics, since it stands as yet the sole representative of the motor-*excitants*, while we have a number of powerful motor-*depressants*. With regard to its effects on the capillaries, it has been seen in one instance at least, according to German authority, to *contract* the vessels of the frog's foot. This cannot be accepted without further observations, since it is not what we would be led to expect from other experiments, and from clinical observations. That it induces hyperæmia of the cord is an accepted fact, and it is quite common to use a very small hypodermic injection as a diagnostic, when we are in doubt as to whether we have anæmia or hyperæmia. Clinical observation would also lead us to infer that it produces dilatation of the cerebral vessels, but I believe that no *experiments* have been made in this direction. I would here remark that probably some of the many discrepancies which are noticeable among experimenters of equal ability, with these active poisons, may be accounted for by the

¹ Others have noticed this interference with the motion of the corpuscles, when experimenting with other agents which contract the capillaries, as digitalis and ergot; evidently due to the narrowing of their calibre, and finally, in the case of digitalis, to complete closure. I have said it is singular that Binz did not remark upon the contraction of the arterioles, if there was any, but he was looking for something else, namely, the direct effect of the quinine on the corpuscles, and of course attributed their tardy movement to this.

fact that the effect on the lower animals, of a *large* dose, which is generally intended, and an *overpowering* dose, or an *overwhelming one*, is often very different, and sometimes directly the reverse. This very drug affords an example, as the following quotation shows: "As Fraser has discovered for atropia, Vulpian has found for strychnia; namely, that after a time, say from some hours to two days, if the dose has been of the right size, the strychnic *paralysis* passes off, the motor-nerves are found to have regained their power, and the convulsions reappear and continue for hours or days." II. Wood (op. cit.). We have also seen, in the first part of this paper, that a certain degree or extent of impression on the surface of the human body may cause a *contraction* of the visceral vessels, while an impression, greater in degree or extent, may cause *dilatation* and rupture. That the influence of this drug is entirely exerted on the *nerve-tissue* of the spinal cord, in the production of the motor phenomena, and not through the blood, on the nerves themselves or the muscles, is well established by numerous experimenters.

Dr. S. Weir Mitchell informs me that he has lately performed the following experiment. Having bled an animal nearly to death, and also isolated a portion of the cord from its vascular connections, he applied strychnia to it, and its peculiar action was well pronounced, showing its action on the *nerve-cells*. We may therefore deduce this valuable therapeutical inference, that in cases of spinal or cerebral anæmia with impairment of nutrition of nerve matter, we may employ this remedy for a double purpose, to act *directly*, and also indirectly by increasing the supply of blood. The prompt and direct conveyance of the strychnic influence from the surface, through the afferent nerves to the spinal cord, and thence to the most distant parts, is proved by the often-repeated experiment of isolating the sciatic nerve in the frog, by a section of all other tissues, and then applying the poison to the foot. It is promptly conveyed to the spinal cord, and its effects manifested in the other limb and elsewhere. I refer to the two following clinical observations as illustrations of the value of the application of the results of this experiment to actual practice.

The treatment of the following case I had the opportunity of watching, through the courtesy of Prof. Hammond. The history I had from the patient himself.

CASE I.—Mr. W., a grain inspector of Chicago, was attacked three years ago with epileptic convulsions; has had them once a month or oftener; also some threatening cerebral symptoms; had no treatment that he knows of except moderate doses of bromide of potassa and chloroform inhalation. In June last he had a recurrence of cerebral symptoms, insomnia, pain, double vision, etc. This lasted two weeks and disappeared. On the 14th July, after some exposure to the sun, he was again attacked with the above symptoms, to a greater degree, and with complete inability to raise the eyeball or upper eyelid (left eye) also extreme internal strabismus diplopia and severe cephalalgia. These symptoms occurred suddenly in the night. Could neither read, nor distinguish the quality of grain. The strabismus disappeared slowly, and the ptosis also diminished somewhat, so that when he applied to Dr. H. about the 13th October, 1874, he could, *by an effort*, raise the lid so as to expose the cornea, but it fell back immediately, other symptoms the same. He was put upon increasing doses of the iodide of potassium with the idea of relieving the *basilar meningitis*, presumed to be the cause of the symptoms, the application of the induced current to the brow and temple, and the hypodermic injection of strychnia. No *immediate* effect could be expected from the first two remedies; it is to the last that I desire to direct attention. Prof. Hammond proposed to inject the solution directly into the affected muscles, and accordingly did so, using gr. $\frac{1}{2}$ in two drops of water; it is presumed that it passed into the muscle, or most likely in its immediate proximity. In all, six injections, I think, were used. I watched the effect carefully and tested the eye and lid after each. They were done each alternate day. He declared that he perceived quite a decided effect. After the second there was no doubt, as I could see the change within fifteen minutes both on the ball and on the lid, but especially on the latter; after the third, the *ptosis* had entirely disappeared, and he could raise the *ball* to a horizontal plane; the diplopia had disappeared, and he could read by

holding the book low. After the fifth injection (gr. $\frac{1}{20}$) no difference in the appearance of the eyes was distinguishable, and he could read with the book held directly before him. He considered himself cured.

A case of almost perfect deafness, incapacitating the patient entirely from business as a lawyer, partly from Eustachian trouble and partly from an affection, apparently of the auditory nerves, was treated by Dr. Hammond under my observation with a striking effect. I could converse with him, after two or three injections, at a distance of two feet, with the voice slightly raised, and speaking very distinctly, no difficulty; but, in cases of deafness, it is so easy for a patient, who is anxious to improve, to deceive himself and others, that I should wish to see or hear from him a month afterwards, before reporting a cure. He, however, returned to his home in Buffalo, with his brother, both convinced of very marked improvement, if not cure.

CASE II.—(From the private note-book of Dr. C. R. Agnew—notes abridged.) *Amblyopia, from tobacco and alcohol.*—On testing vision, it was found to be $\frac{14}{100}$ with either eye, and not improved by glasses. Both visual fields perfect, and no color scotoma. On ophthalmoscopic examination, both eyes emmetropic, and in each eye that dirty, ill-defined appearance of the nerve and retina, which, taken in connection with diminution of the vision, without other evident cause, usually leads us to suspect alcohol and tobacco poisoning. Directed him to abstain entirely from alcohol and tobacco, and to return in a week. Oct. 30, one week later, R. E., V. $\frac{20}{100}$, L. E., V. $\frac{20}{100}$. Injected strychn. int. gr. $\frac{1}{36}$, and in one minute vision rose to $\frac{20}{100}$ with both eyes; and five minutes later to $\frac{20}{100}$. The infection was in the cellular tissue, but not in that of the affected part. In Dr. Hammond's ear case, it was in the arm. I do not suppose that the medulla spinalis was affected through the rapid absorption into the blood, but that the effect was transmitted to it, as electricity would be, and as, in the case of the sciatic nerve of the frog, almost instantaneously. Nor do I suppose, even if the solution reached Mr. W.'s superior rectus muscle, which it probably did not, that it affected the muscle *directly*; for it is now

an unquestioned fact, that if the leg of a frog is amputated, and a solution of strychnia injected into the artery, no effect on the muscles follows. It must take place, if at all, through the spinal cord, and cannot reach it through the blood, as has been demonstrated by the late experiment of Weir Mitchell, and by others.¹

The effect of *atropia* on the vaso-motor system, and probably on the nerve-tissue itself, is scarcely less interesting and important than that of ergot and strychnine, but I am compelled to abstain from the more thorough analysis of its physiological and therapeutical action, as well as that of the other valuable drugs enumerated in the list, in an early part of this paper which was proposed; in consequence of its unexpected length, which was wholly unintentional, but could scarcely have been avoided. For the same reason, it will be advisable to defer reading the *cases* appended to this paper, and which were relied upon, in some measure, to corroborate the facts and opinions here and there given, without a due amount of evidence. With ergot, atropia has been lately relied upon as our best resource for reducing the calibre of the capillaries, and thus relieving inflam-

¹ As illustrative of the action of strychnine in these cases, I refer to a case published by me in the *Am. Jour. of Med. Sciences*, 1862, of injury to the branches of the supra-orbital nerve, by a fragment of copper cap, causing partial amaurosis, and immediately relieved by removing the foreign body; also other cases of amaurosis caused by trivial cicatrices of the scalp, and relieved by excision. Handfield Jones states cases of paralysis of the muscles of the eye from neuralgia. (*Practitioner*, March, 1874.)

Mr. Charles Hunter, of London, who has written largely on hypodermic injections, makes a statement calculated to injuriously circumscribe the applicability of strychnine in these cases, to the effect that if improvement does not follow three or four injections, the case might as well be regarded as hopeless, so far at least as this remedy is concerned. This statement is contradicted by my own experience, by that of Prof. Hammond, especially in cases of infantile paralysis, and by several American oculists. In fact the proper dose cannot be ascertained until several have been given, as the toleration of different patients differs materially.

The effect of the little gelatine wafers, used by oculists to dilate the pupil, is similar to that claimed for strychnine, otherwise it seems incredible how the one-hundredth part of a grain of atropia should act, as it does almost immediately. If it is claimed that it acts through the blood, then the disciples of Hahnemann might well claim this as a proof of the efficacy of any of his attenuations.

mation. The physiological experiments on animals are very numerous, and the number of distinguished experimenters large. An excellent *résumé* of these interesting experiments will be found in H. Wood's work, so often referred to, which conclusively demonstrates the fact of a decided contraction of the arterioles, not from the direct effect observed under the stimulus of atropia on the frog's foot, or the mesentery, but mainly from the uniform increase of *blood-pressure*, from contraction of the capillaries, and the absence of this after section of the medulla. Dr. Wood has also repeated these experiments.—*Am. Jour. of Med. Sciences*, April, 1873. One of the clinical histories, which were to have been appended to this paper, affords a striking exemplification of this fact. The excessive congestion of the lungs, and extravasations therein, and even of the retina itself, shown in post-mortem examinations of those dead from immense doses of atropia, indicate, as has been indicated in the case of other alkaloids, the importance in estimating their physiological effects and therapeutic value, of discriminating between the phenomena produced by moderate and by inordinate doses respectively. Atropia has lately been highly recommended in excessive sweating, especially of phthisis; we all appreciate how annoying and intractible a symptom it is, especially since we cannot usually remove the cause. Supposing it to be due to a vaso-motor paralysis of the *plexus* supplying the sudoriparous glands, we should expect a remedy like this to act promptly on its nerves, diminishing the lumen of the vessels, and thus arresting the hypersecretion more certainly than the ordinary astringent remedies usually given, and which act through the uncertain medium of the blood, in which they must necessarily be largely diluted. Besides this, our most reliable remedies are very *hot* water to the surface, producing a reflex action, and the old-fashioned sage-tea. Had we the active principle of the sage, or volatile oil, so as to employ it in more definite and reliable doses, it might succeed even better. The great value of atropia in opium-poisoning is now so generally admitted, though still denied by many writers, that it is hardly necessary to allude to it. I have published several striking instances myself, and would refer any skeptics to Dr.

Trask's account of his own case (*N. Y. Med. Journal*, Nov., 1874). He took a dose, by mistake, estimated at six to seven grains; and I heard him read an excellent paper recently before the Obstetrical Society of this city, indicating no damage to body or mind. Electricity and atropia saved his valuable life.¹

The consideration of *opium* naturally succeeds that of *belladonna*. M. Secé has lately placed morphia with strychnine as a "motor of the spinal cord." Recent extensive experiments in Germany go to show that the local application of morphia increases and protracts the excitability of the nerves induced by strychnia; and its action on the brain, in certain doses, not too large, is similar to that of strychnine. Opium has not yet been proved experimentally to dilate the capillaries, but clinical observations lead us to infer that it does. We have seen how prompt is the action of belladonna in the relief of sweating, and we know how certain the action of opium is in its production. The idea that opium does produce hyperamia of the brain has had a firm hold on the minds of the profession from the earliest period, and has had a very unfortunate influence on the treatment of cerebral affections, especially in childhood, and on a large number, perhaps, whose convulsive phenomena are due to mere *irritation* of the meninges from a morbid influence applied to the peripheral nerves in a distant part of the body, generally in the abdomen. Though opium may physiologically dilate the cerebral vessels, and though a positive inflammation of the encephalon may exist, if the pain is excessive, the restlessness and insomnia protracted, and uncontrollable by other means, as cold, bromides, atropia, etc., as is too commonly the case in acute forms, opium is our only resource; it may have to act in some desperate cases, by its overwhelming effect. But it may generally act in a legitimate and rational manner. The old

¹ Two remarkable cures by belladonna of exophthalmic goitre, in which the symptoms had assumed such proportions as to threaten death, are reported in the *Lancet*, 1874, by Dr. Smith, of London. Various other remedies had been used in vain. Profuse sweating in both was a prominent symptom. Five drops of the tincture were given hourly, and there was a rapid and progressive fall in the frequency of the pulse. At the end of ten days a drachm was given daily.

maxim, *ubi irritatio ibi fluxus*, will explain it. The very pain and its consequences cause or increase the flow of blood, and the relief of pain and the allaying of excitement, restore, to a considerable extent, the balance of the circulation. The truth of the above maxim is well illustrated in facial neuralgia, which cannot be charged with being inflammatory; when excessive the face becomes red and swollen, looking as if it had been struck; a full and effective anodyne relieves the hyperemia as well as the pain. No matter what the disease, whether attended by fulness or anemia of the vessels, if pain or spasm is uncontrollable by other means, opium is a proper remedy. Even in the profound coma of *uremia*, opium, by allaying the irritation of the brain, produced by the poison, will rouse the patient out of it. In the convulsions and coma attending or following the puerperal state it is our best remedy; though, when the patient is in good health, the old plan of bleeding, no doubt, aids materially, by removing a portion of the poison with the blood. It is hardly necessary to say that, in all these cases, the action of opium is on the nervous system, and has no influence whatever in the blood, which is its vehicle merely.

I have included the *anæsthetics* in the list of neurotics, because their action, however we explain its rationale, is on the nerve-centres; it is through them that they produce all their marvellous effects, and it is to them that we must look for explanation of the fatality which attends the use of some of these agents, and to which we must direct our means of resuscitation when death has occurred, or is imminent. On the capillaries, both ether and chloroform have been proved, by experiments and observations, to act like atropia, ergot, etc., to contract them; we need not fear to give them, therefore, in congestion or inflammatory conditions, though the struggles, which are apt to precede full anæsthesia, may temporarily increase the fulness of the cerebral vessels. Various explanations of the cause of anæsthesia, and of death from it, have been given, but none of them are thoroughly satisfactory. In the few cases of death from ether, it has been noticed that respiration is first arrested, and some time after, the action of the heart. The reverse is the case with chloroform; with

ether, the effect is slow, and preceded by symptoms, which, with watchfulness, might enable us, perhaps always, to ward off a fatal result; with chloroform, it comes like a stroke of lightning, the heart is paralyzed in an instant. How can so sudden an action be explained? Only I think by a reference to a reflex influence on the nerve-centres. Because the application of pure chloroform to the naked heart, or the injection of pure chloroform into the jugular vein, has produced instant paralysis, it has been assumed that the paralysis in inhalation is also caused by direct action through the blood; it is surprising that one who reasons so well, and analyzes so well as Horatio Wood, should adopt this explanation. The charging of the blood by any anæsthetic must, under any circumstances, be a slow process, and a sudden effect could hardly be expected; and the paralysis, produced by the direct application of so irritating an agent as chloroform, whether to the surface of the heart, or internally, through the jugular vein, is more likely caused by reflex action, which would just as soon follow ammonia or nitric acid; just as when atropia was applied to the frog's foot and contraction of the capillaries followed, it was assumed that it was from its direct action on the vessels, but subsequent experiments, with the skin above the foot divided, showed that it was from reflex action. On what peripheral-nerves, then, does the chloroform act to produce so powerful a reflex influence on the nerve-centres and thence on the heart? I answer on those of the air-cells and the bronchioli; and the reason why it does not *always* produce this effect is that it is generally largely diluted with air; and, a greater protection still, because the air, entering the lung at each inspiration, bears but a small proportion to that already in the lung, the *residual air*, and this therefore can be but slowly charged. But this care in diluting the anæsthetic with air, notwithstanding the evidence which is so satisfactory to a juror's jury, is, as we all know very well, sometimes forgot—ten in the interest of the operation, and, now and then, I fear, in sheer recklessness, and the full force of the vapor, by an unusually forcible inspiration, is carried deeply into the lung, and, acting on nerves already pretty thoroughly impressed,

induces the sudden reflex phenomenon which we frequently see exemplified by other accidents on other parts of the system. Walshe says: "Saturation of the nerve-centres is the essential and radical cause of the event." This is high authority, but he does not advance any proof, and the effect is too sudden to give the idea of saturation, through the blood. Whatever the impression may be, it must be something sudden and powerful, generally acting on tissues already tolerably saturated, but not necessarily so, as the effect is sometimes produced at the commencement of the inhalation. Paralysis of the heart is an exceedingly common accident from a great variety of causes, mental and physical, and occurs as the cause of death, and often as the unexplained cause of death, in many diseases. It occurs in animals as well as in men, from mental emotion alone; in the former from fright; of which there are numerous examples; in the latter, from pleasurable as well as painful emotions. Of the former, a little bit of revolutionary history will serve as an example. When the surrender of Lord Cornwallis was announced suddenly to the door-keeper of Congress, he fell dead. Similar examples are on record. Fright, we all know, will suddenly, by acting on the nerve-centres, relax the sphincters, and act on the bowels, and also produce sudden death. A blow on the epigastrium, transmitted to the semi-lunar ganglion, or the crushing of an extremity (Walshe) will paralyze the heart.¹ The effects

¹ As additional evidence, for those who require it, of the important and immediate influence we may exert, by our remedial applications, whether by heat, cold, mechanical violence, as by cupping, electric shocks, etc., or by drugs. I refer to p. 437 of Flint's *Physiology of the Nervous System*. "Brown-Séquard and Lombard, found that pinching of the skin on one side was attended by a diminution of the temperature in the corresponding member of the opposite side; and that, sometimes, when the irritation was applied to the upper extremity, changes were produced in the lower extremity. Tholozan and Brown-Séquard found also that lowering the temperature of one hand produced a corresponding depression in the heat of the opposite hand, without any notable diminution of the temperature of the whole body. Brown-Séquard showed that, by immersing one foot in water at 41° F., the temperature of the other foot was diminished about 7° F., in the course of eight minutes." These facts show that certain impressions made upon the sensory nerves affect the animal heat by reflex action. Also that the application need not be made in the immediate vicinity of the part designed to be influenced.

of the inhalation of ether and chloroform respectively, and likewise of the slow and sudden inhalation of chloroform, may be likened to those of the gradual, and the sudden annihilation of the brain, as detailed in the experiments of Legallois, in which he showed that the heart continued to beat after he had removed the entire brain, piece by piece, *slowly*. But, that instant paralysis followed *sudden* crushing of the encephalon. This may, I think, be considered an *experimentum crucis* in sustaining the theory advanced in this paper as against that of the direct action on the heart (Wood) or the gradual saturation of the nerve-centres (Walshe).¹ Flint says that sudden or powerful galvanization of the *pneumogastrics* will so excite their inhibitory action as to endanger heart paralysis. "The effects of pressure on those nerves in the human subject," says Flint, "is described by Aristotle, quoted by Waller. In some cases, the patient falls instantly, as if struck by lightning, while in others, the effects are not so marked." Waller performed several painless operations under this mechanical anæsthesia.

Dr. Marion Sims, in his paper recently read before the British Medical Association, attributes the danger to *cerebral*

¹ B. W. Richardson says death is produced by the action of the vapor on the peripheral nervous system. But, he explains it in this way. The inhalation suspends respiration "for an interval, then accumulation of carbonic acid in the blood, then irritation of the vagus, then arrest—from the irritation—of the action of the heart."

Why not irritation of the *vagus primarily* (by the chloroform) rather than *secondarily* (by the carbonic acid) since the former is certainly more irritating than the latter? And the sudden character of the death would rather indicate a direct than an indirect process. It ought not to be forgotten, in estimating the effects of chloroform, that it *directly* tends to produce cerebral anæmia, by contracting the vessels; as does ether also. It is probable that force and fulness of the heart's pulsation, and diminished frequency, so commonly consequent on full anæsthesia, is due to a general contraction of the arterioles or the capillaries, and a consequent rise in arterial tension, and that the remarkable feebleness of the heart's action, which not uncommonly follows recovery from anæsthesia, is due to the sudden expansion of those minute vessels, and rapid drain on the encephalon. Here the nitrite of amyl would seem to be *the* remedy. Max. Schueller's recent experiments on animals demonstrate that this drug can remove the effects of chloroform on careful vascularity very promptly.

anæmia, and highly extols Nélaton's plan—suspension with the head downwards, by which a patient of his was restored to life. Of all single means of resuscitation this is probably the best. But we must go behind the *anæmia* to find out the source of danger; for this is due, as has been suggested, to cardiac paralysis, and behind this is the fatal chloroform vapor acting through the peripheral nerves (of the pneumogastrics); so on the nerve centres, especially the *medulla oblongata*, exciting their inhibitory function, through the *par vagum*, on the cardiac ganglia and plexus. So that while we invert the body, we should also endeavor to rouse the heart's action by electricity applied to the pneumogastrics; and by applying the electrodes just above the clavicles, on either side of the neck, we may act upon the phrenics at the same time, thus exciting the diaphragm, and inducing respiratory efforts. This, in all probability, would have saved Dr. Sims and his illustrious confrère a world of dreadful suspense. With these measures might also be combined the injection of stimulants *per rectum*, or *intra-venous* or even of ammonia, after the manner of Halford, the use of digitalis, and of nitrite of amyl.

Cardiac paralysis, or a tendency to it, *cardiac asthenia*, as Fothergill¹ aptly terms it, is so common and so dangerous a complication of a great variety of diseases that it demands further notice. Many times have physicians been astonished and shocked at a sudden and inexplicable "turn" for the worse, or a sudden death, in a case of acute disease, when all seemed to be going well; and their astonishment has not abated when, on a post-mortem inspection, they have been able to discover no sufficient cause of death. Two cases have, within the past two weeks, occurred in my own practice, which may serve as examples:—

CASE I.—L. T., a stout, healthy, temperate young man, had single *pneumonia*, and was attended, in my absence, by Dr. J. C. Young, late Resident Physician of Bellevue Hospital. The case progressed as usual, and in the course of a few days reso-

¹ "Digitalis: its Mode of Action and its Use." By J. Milner Fothergill, London.

lution commenced; then the other lung was invaded, but he showed no unusual or alarming symptoms; was taking stimulants very moderately. At 9 o'clock P.M. was seen by the doctor; pulse, 104; fair strength; respiration not particularly bad. At 12 P.M. he became weak and restless, then delirious. The doctor was called at 4 A.M., and found him with a feeble, frequent and irregular pulse, and he died in almost twenty minutes, although he swallowed the stimulants freely given.

CASE II.—Notes furnished by Dr. Murdock, who treated the case.

October 23. Mrs. M. C.—seventh day of single *pneumonia*. Doing well, and resolution commencing. Had taken nourishment abundantly, and had not required any stimulants. To-day found her pulse had risen from 100 to 116, and was somewhat feebler. Ordered brandy, $\frac{3}{4}$ iij. daily. Called in haste to her at 9.30 P.M. Found that she had steadily grown worse through the day, and for some hours alarmingly so. I found her almost pulseless, the heart's action too feeble, rapid, and irregular to number. Surface bathed in cold sweat, extremities cold, inspirations frequent and shallow, mind wandering. Ordered heat to extremities and warm blankets. I gave three drachms of brandy every ten minutes. This was kept up at intervals of fifteen to twenty minutes, for nine hours. She also had a half ounce in beef-tea, *per rectum*, every hour and a half, and *ammon. carb.*, gr. x, 2 h. Her case seemed utterly hopeless for some hours. It was not until midnight that the skin became at all warm, and the pulse sufficiently strong to be numbered, still irregular. At 1.30 pulse 120 and more regular; skin warm. October 24, 9 A.M.—Stimulants have been continued steadily in same doses. Pulse 120, but quick and feeble. Three P.M., patient becoming hot and flushed; the brandy was given only every half hour, and the enemata were not retained, and had to be discontinued. Now, pulse feebler, and constantly intermitting. Continued brandy every twenty minutes, carbonate of ammonia and also *tinct. digitalis*, m. x. q. 2 h. Six P.M., improving, pulse more regular; to give brandy every thirty minutes and *digitalis* every hour and a half. October 25, 8 A.M.—Much improved. Pulse 108, and quite regular. Brandy

as before. Digitalis q. 2 h., and stop ammonia. Eight p.m., improving; digitalis q. 3 h. October 26, pulse 98; stop digitalis. From this date she continued to improve regularly. Attention is called particularly to the fact that there was no apparent change in the condition of the lungs to account for the sudden failure of the heart, and that, with the constant administration of stimulants, by mouth and rectum, for twenty-four hours, no marked improvement resulted until the digitalis was commenced, and almost immediately a favorable change was inaugurated.

Dr. Young calls to mind eight cases of pneumonia, two noted as double; and his successor remembers two others, and a case of Bright's disease, during his term of service (two years), in which sudden death occurred while the patients were walking to the water-closet, or merely getting on their feet, and of course not considered in a particularly dangerous condition. A case is related in the August number of *The New York Medical Journal*, as having occurred at the Charity Hospital, in which sixty-four ounces of serum had been drawn from the pleural cavity by aspiration, and eight hours after, the patient having been in the meantime comfortable, and sitting up part of the time, fell dead while walking to the water-closet. Prof. Hammond lost two cases, while in the army, in this manner; and a lady, under his treatment and that of Dr. Sims, for the opium habit: she died while walking across the floor. In convalescence from diphtheria, and during the attack, though the patient may not seem particularly weak, and able to be about the room, sudden death from this accident is very common. The first child I ever saw with it died during convalescence, from *taking her up*, contrary to express order, to pass water; and another, whom I had seen a few hours before walking about the floor, but with patches of dense membrane covering the pharynx, fell dead while walking from the sofa, on which I had directed he should be kept in the recumbent posture, to where his father sat, a few feet away. A lady, upon whom I had performed artificial delivery for convulsions, three days before, who was progressing favorably, and who was expressly ordered not to raise her head, feeling pretty well, sat up to

take some drink, and fell back gasping, and was dead in an instant. Dr. North, then of Bellevue Hospital, who was assisting me in my practice at the time, was promptly by her side, but she could not be revived. She was excessively pale. These cases are all recent, and might be multiplied by consulting the journals and records of hospitals. Of course, some will attribute this accident to heart-clot. The autopsies in the hospital cases only showed clot in one. In diphtheria, we know that paralyzes, in all parts of the body, are common. And the appearance of the patient's countenance in the other cases indicated paralysis rather than clot. Besides, when we do find clot, it is probable, in most cases, *post-mortem*, or forms in the death struggle.

"In this condition," says Fothergill (op. cit.), "we find the pulse furnishing us with the precursory evidences of approaching death." A very frequent, a weak, and an irregular beat. Clinical evidence informs us that this is most apt to occur in thoracic diseases, especially pneumonia. Why it is so is very evident, and it is hardly necessary to repeat the very lucid explanation of Fothergill. Suffice it to say, the lungs imperfectly aerate the blood, which flows *slowly* into the left ventricle, while the right becomes distended. "We have the blood more than ordinarily laden with carbolic acid, an agent which has a direct effect in paralyzing the heart, when brought in contact with the endocardium." An experiment of Cyon will illustrate this. He passed a stream of serum charged, and then one not charged with carbonic acid, and thus paralyzed and then restored the contractions at will.¹ There are all grades of this *cardiac asthenia*, from impending paralysis to a point where the want of correspondence between the state of the pulse and other symptoms is barely noticeable; and between these two degrees will be found a large proportion of cases, in which a remedy directed expressly to this peculiar heart-trouble may strike the balance between life and death, or may greatly curtail the duration of convalescence. It is in these cases that stimulants act so well, so greatly insisted on by Hughes Bennett. But frequently stimulants are too slow; and here digitalis, even by hypoder-

¹ Sydenham Soc. Year-Book, 1867-68.

mic injection, if necessary, proves its great superiority. "The question of special stimulants to the cardiac ganglia has," says Fothergill, "scarcely ever been broached." In these desperate cases, we must remember that *opium* also may afford material aid, and that it seldom acts, unless in unreasonable doses, otherwise than favorably on the heart; and if jactitation or pain be present, a hypodermic injection should be used.

I had a most remarkable exemplification of the value of opium in advanced heart-disease a few years since. An old man, known to everybody in the neighborhood of Cold Spring as Johnny Moon, had severe valvular disease of the heart for nine years. Finally dropsical symptoms set in, and he took to his bed, and after having exhausted all the ordinary remedies, with temporary relief, finding his sufferings very great from the great amount of effusion and debility, I gave his wife a solution of morphia, to be used *pro re nata*, and no other remedy. I did not happen to see her for several weeks, when I met her, and to my inquiry she said, "Oh! doctor, that last medicine is curing him." And, surely enough, the effusions everywhere had entirely disappeared, and for some years after he was apparently quite well. But the heart-sounds were, of course, unchanged. I was in the habit of exhibiting him to my medical friends as a curiosity. He did not, after all, die of heart-disease or dropsy.

The cases of delirium tremens to which digitalis is applicable, and for which doses of two and four drachms even have been prescribed successfully, are those of cardiac asthenia, and indicate the boldness with which it must sometimes be given. Here the hypodermic injection would probably be safer. In very dangerous cases of suppression of urine, when digitalis, in the usual doses, fails to act, I have used poultices of digitalis, made by wetting flannels in a very strong infusion—four ounces to a quart—and using all the leaves (and none but the best English should be used) in one poultice, which extends all around the body, and from the thorax to the pelvis. H. Wood cautions against the danger of making the application too extensive and too strong; but I have used it as above, both in children and adults, and never saw any ill effect. However, no one should employ any of these powerful agents, now under discussion,

"without bearing in mind that he is dealing with a two-edged and a very keen-edged weapon; and that, when we speak of giving these fearlessly, and in such bold doses, it is generally in cases of imminent danger, where delay or pattering treatment is the most hazardous of all.

The *nitrite of amyl*, a remedy of recent date and not yet officinal, has already been alluded to in connection with the effects of other drugs. It is a most valuable addition to our neurotics, since its action on the nerve-centres is prompt, uniform and decided, and, with moderate care, it is quite safe; patients afflicted with epilepsy or *angina pectoris* being constantly trusted to use a regulated dose themselves by inhalation. In all spasmodic conditions it is invaluable. Prof. Janeway recently related the particulars of a case occurring at Bellevue Hospital, of a patient "moribund from advanced Bright's disease. He was unconscious, pulseless, pupils widely dilated, no sensitiveness of conjunctiva; twenty-five drops restored consciousness. The pulse gradually became full, and the patient could talk," and seemed like one restored from death to life. This is precisely similar to the effects produced by the transfusion of moribund cholera patients with saline solutions. The truce is but a brief one, however. Its importance as an auxiliary in the cerebral anemia of chloroform poisoning, and of other similar and not necessarily fatal conditions has already been noticed. As strychnine is used to diagnose doubtful conditions of the circulation in the *cord*, so we may use this drug for the *brain*, and Weir Mitchell¹ has used it for this purpose, Prof. Hammond also. Quite recently I have seen two cases in the practice of the latter, of epilepsy, in which the drug evidently precipitated the convulsion, and it was necessary to abandon it. Crichton Browne, in his large experience with this remedy has, I believe, met with no such cases. This drug produces three most marked physiological actions, and with remarkable celerity and uniformity; first, dilatation of the arterioles, consequent sinking of blood-pressure, and of the action of the heart; secondly, loss of functional power, even to complete abolition, of all muscular tissues; thirdly, remarkable

¹ Medical and Surgical Reporter, July, 1874, p. 60.

fall of temperature. Dr. H. Wood (op. cit. p. 299) says: "I have seen a pigeon perfectly conscious, although his temperature had been brought down by this agent some 13° F." As he states, that this effect is equally marked in fever, it may prove valuable as an antipyretic. This remarkable fact seems, at first glance, to be in direct conflict with the theory supported in this paper, that the rise of temperature in fevers and other diseases is due, in a great measure, to a dilatation of the smaller vessels, arterioles and capillaries, to a more rapid consequent metamorphosis of tissues, and that the action of our antipyretic remedies is mainly through their constricting effect on these vessels. But a *fourth* very remarkable property which this singular remedy has been proved conclusively to possess, harmonizes this apparent antagonism. It had been observed that in animals poisoned by it, the color of the arterial and venous blood was almost the same. Dr. Gamgee, by a series of beautiful experiments, has shown this to be due to the power of nitrite of amyl, in almost completely annihilating the oxidation of the blood corpuscles, and, to that extent, interfering with tissue change and the production of animal heat. A few drops of amyl diffused through a jar containing glowing phosphorus will extinguish it at once (Wood).

Three, four, or five drops by inhalation, from a piece of lint or a morphine bottle, constitute a dose for the most violent diseases. A case of puerperal eclampsia was arrested, as the *convulsions* would come on, by a single drop. But the secondary effects must be looked to here, as in chloroform or other inhalations under similar circumstances, namely, the danger of post-partum hemorrhage. To *tetanus*, in connection with the physostigma or conia, and the bromides, it would seem to be particularly applicable, and some experiments on animals confirm this.

The action of *iodine* on some nervous diseases, and especially on the nervous manifestations of syphilis, is so remarkable and important as to justify considerable attention, if this paper had not already assumed unwieldy proportions. I will merely ask your attention to the fact that, though the iodide of potassium is a specific in these affections, and the bromide for certain other

nervous maladies, neither the iodine nor the bromine, nor the potash alone will have any decided influence whatever. Nor do we, as a general rule, see a *gradual* amelioration of the symptoms, but, after giving one, two, three, four, five, and sometimes six or more drachms of the *iodide* per day, in gradually increasing doses, we see a sudden and very marked beneficial effect. And yet, if, while this tentative process is going on, we test the urine and other excretions, we find our remedy passing out of the system almost as fast as it is passing in, and, therefore, apparently not producing its effect by cumulative action. In the present state of our knowledge of the subject, there would seem to be but this explanation—that the iodine, when given alone, becomes rapidly acted on by the various chemical agents with which it comes in contact before reaching the nervous system, but, that, in the form of iodide, it is *protected* from change until it reaches its ultimate destination, where, under the influence of changes going on in the process of tissue formation, the salt is decomposed, and iodide set free. It has been shown that medicines in a *nascent* state have a peculiar effect. Could we, in any particular case, know the exact dose without trial, very few of these efficient doses would doubtless effect a cure; since it is evidently not the blood on which we act, and, moreover, during the time in which we are finding our dose, we are interfering with the nutrition of the tissues. I may also instance the effect of large doses of the iodide of iron in some obstinate forms of hemorrhage, especially uterine, and in nocturnal enuresis of children. It is not always that iodine requires to be administered in these very large doses to produce its decided effects on the nervous system. For instance, in spasmodic asthma, I have repeatedly relieved the most violent attacks, after failing with various remedies, by a dose or two of four or five grains, and within an hour or two. The noted Jonas Whitcomb's remedy probably owes all its success to this drug.

The *bromides* have already been incidentally alluded to. There is one rather widespread error, however, regarding their action, which tends greatly to circumscribe their usefulness, namely, that in cases of cerebral anemia, or a condition of the

circulation in which it may be supposed that this exists, they are useless and even injurious. For instance, in an abstract of a paper on the bromide of potassium, in a recent number of the *New York Medical Record*, by Dr. Carr, he enunciates the prevalent idea thus: "If the brain is already without the proper supply of blood, the extra amount which will be removed from it by the action of the bromide of potassium, will not only not benefit the patient, but will probably increase the force of the paroxysmal seizure." Now, the results of the experiments with this drug on animals carefully collated, as well as those of clinical experience, go far towards demonstrating the fact that the least important of the effects of these agents is their vaso-motor influence; that their most important action is in controlling, and, if pushed far enough, almost obliterating *reflex* actions. First, by a direct effect on the nerve-centres, the medulla oblongata and cord especially; secondly, on the *peripheral* nerves, that is, blunting the susceptibility of the latter to painful impressions, and also the receptivity of the cord as to these impressions.¹ I refer to one corroborative clinical fact. In the case of a lady who had bled to the very verge of death from metorrhagia, who was insensible, and at one time pulseless, but who, upon partial recovery, fell into an alarming condition of nervousness and insomnia, which failed to yield to opium, chloral, etc., I suggested to my friend the late Prof. G. T. Elliot, who was sharing the responsibility with me, to try the bromide of potassium. He at first scouted the idea, but finally agreed, and we were gratified to find that it acted as it ordinarily does in cases supposed to be suffering from hyperæmia.

This important virtue of the bromides is well exhibited in preventing sea-sickness, and the vomiting induced by anæsthetics, and the unpleasant effects of opium and quinine, which clinical experience has recently taught me. But this property has special applicability in the treatment of certain functional

¹ It has been shown by experiments on animals, that the effect of the bromides applied directly to the nerves is very decided, and that it has therefore an effect on the peripheral nerves independent of that through the nerve-centres.

diseases of the heart, the history of a case of which, now under treatment, will serve as an illustration.

A. M., a young man of fair constitution, but of a family decidedly addicted to insanity and neurotic affections, applied at my office about four weeks ago, during my absence from home, and was seen by Drs. J. C. Young and G. W. Murdock. He had left his home in the West on account of heart trouble, which had annoyed him for some months; he described it as a fluttering, and a painful consciousness of its action, the impulse being very strong. This was increased by exertion of any kind. He is not addicted to excess of any description; takes neither liquor nor tobacco. He looks pale, but is not anæmic; has a good appetite and digestion; sleeps well. Four years ago had what he calls rheumatism, but it was probably myalgia. Complains now of a sort of stiffness across the chest and shoulders anteriorly. Pulse good. They examined him, and found a distinct bruit with the first sound, heard at base and apex; but after conversing with him for a time, and re-examining him, they were surprised at finding no bruit whatever. This happened at two other visits. There was no hypertrophy. On my return, I recognized the case as similar to at least two which I had seen some years ago, and, on making him change his position, the sound would change from base to apex, or sometimes disappear altogether at the same visit. When standing, merely bending well forward, would alter the bruit, or cause it almost to disappear. But these changes did not, at different visits, correspond with the same positions; sometimes the sitting posture developing a louder bruit than the standing; sometimes the reverse, sometimes causing its entire disappearance; sometimes a walk, twice across the office, would develop the sound; sometimes have no effect. This is the peculiarity of the cases, and I do not think that such cases are distinctly referred to in any systematic work. Tobacco causes symptoms somewhat similar. The bruit is louder, and yields readily to abstinence from the cause. It is easily distinguishable from the symptoms of anæmia. One case, I remember, was in the person of the coachman of a friend; he was the picture of health, and had no *symptoms*, if I remember rightly, but presented himself for examination.

Greatly surprised at finding a bruit, and examining him again and again, and in different positions, the same phenomena as above were developed. Seeing me puzzled about him, he said that he had been in the employ of Dr. Wm. Stokes of Dublin, and was examined by him. The doctor, he said, called the attention of O'Brien Bellingham, who happened to be in the office, to his case. Cases somewhat similar to these have been observed by other physicians occasionally, especially in connection with *chorea* and other nervous diseases; but the sound has not been described as varying with a mere change of position, or sometimes with no change at all, at the same visit. Although these marked cases are comparatively rare, the same pathological conditions most likely have an existence, and exert an injurious influence in cases of organic disease, for the apprehension excited by a consciousness of this ailment, especially when operating on a nervous temperament, would be apt to induce such an effect on the nerve-centres, and so on the little cardiac ganglia which preside over special sets of fibres of the heart, as to produce the irregular muscular action, or want of unison in action, which is probably the cause of the phenomenon; thus not only modifying the *organic* bruit, but impairing still further the function of the organ.

To return to the history of A. M., I advised *digitalis* as a *test*, not on the supposition that it was the appropriate remedy, also the acid phosphate and the syrup of the phosphates. In a few days he returned and said he was worse, that the thumping was more distinct. He then informed me that he had taken *digitalis* before, and with the same result. The phenomena on auscultation were not altered. Directed the discontinuance of the remedies, and to take a teaspoonful of cod-liver oil three times a day, and thirty grains of the bromide of potassium three times a day. Four days later he returned, and an examination showed no change. But, on inquiry, it was found that he had neglected to take the bromide. He was now ordered positively to commence this at once. Dec. 3d, three days later, he presented himself, and neither Dr. Young nor I could detect any bruit, in any position in which we could place him, and on repeated examination. This was the more remarkable, if caused

by the medicine, since his mind had been very much disturbed by the sudden death of the mayor of New York, whose symptoms, he imagined, closely resembled his own. Directed the medicine now twice daily. Dec. 5th. Examined the patient to-day with Dr. Young, and although he had been exercising considerably, there was no bruit in any position. There is a want of correspondence, as is often observed in these nervous cases, between the powerful impulse of the heart and the strength of the pulse. Directed to continue the medicine three times a day. We may readily imagine why this drug is so universally applicable in disease, as to draw down, on those who use it largely, the charge of hobbyism, since there is almost no complaint in which, at some stage, there may not be this undue impressibility of the nerve-centres or peripheral nerves, and consequent undue reflex activity, calling for the regulating power of the bromides.

In concluding my remarks on the action of special drugs, I refer to *coniium*, which was fully discussed in Dr. Peter's paper, at our last meeting, only to direct attention to another reliable preparation, and the only one for hypodermic medication; also to a valuable paper by Dr. J. Wilkie Burman in the West Riding Asylum Reports on its use in acute mania, in promptly subduing excessive muscular action. The *succus conii*, so much relied upon at one time in Great Britain, is now pretty much abandoned as uncertain, and inconvenient for its bulk. The fluid alkaloid *conia* has been attempted by their most eminent manufacturers, as Duncan and Flockhart, Morson, etc.; but according to Dr. Burman, with uncertain results, until T. and H. Smith, of Duke St., Edinburgh, furnished their preparation, which, he says, he has always found reliable. The others he found not only inefficient as regards expected results, but dangerous in producing unexpected phenomena. The preparation of the celebrated Morek does not correspond in appearance with the description of the Smiths', and Mr. Nurgard, of this city, has promised to import some of the latter.

As regards the use of these active neurotics, allow me again to advert to the matter of *doses*. It is certain that most of the failure and disappointment, often complained of, has resulted

from inefficient doses. We may, I think, give this as a rule of action: that the drug cannot be said to have had a fair trial unless it has relieved the symptoms or produced its toxic effects. With regard to the bromides, strychnia, opium, atropia, physostigma, this is especially the case; and in very dangerous cases, the toxic effects need often to be very pronounced. The doses, in fact, are as various as the temperaments and idiosyncrasies of the patients. Every one is aware of what enormous doses of opium are borne, in certain conditions of the system, with impunity. Large doses of some of these drugs have already been adverted to, and I refer to a very interesting case in the last number of the *London Practitioner*, for an account of the successful employment of immense doses of physostigma, the patient a physician, and a man of note. He took, in four days, of the solid extract of a reliable manufacturer, sixteen, forty-eight, fifty-seven, and seventy-two grains. At one time serious paralysis was induced. But the urgency of the disease, tetanus, required a perseverance with the drug.

As many of the topics discussed in this paper are still considered *sub judice*, as the observations, which the author has had the honor to present this evening, are intended rather as suggestive than as demonstrative, and, as he is aiming at truth rather than the establishment of theory, he invites your most unrestricted and impartial criticism, and holds himself ready and anxious to receive correction and instruction on these most obscure and difficult problems of medical science.

COLD SPRING, Dec. 6th, 1874.

NOTE.

Since this paper was written, other cases, so strikingly illustrative of the correctness of the opinions expressed by the author have fallen under his notice, that he feels justified in adding them in the form of a postscript.

His attention was called to the first case by Dr. Marion Sims. A member of this Society, on going to visit a patient, got his

feet and legs wet in a violent rain; he sat with the patient for a couple of hours, and soon after his return home was attacked with a violent chill, and all the usual symptoms of intermittent fever. He has since continued to have a regular recurrence, notwithstanding appropriate treatment. He has lost a great deal of flesh and strength during the attack. When we take into consideration the fact that, as was understood, he had never previously had the disease, that he was in perfect health at the time, the conclusion seems almost irresistible that the exposure was the most important, if not the only factor in the outbreak. Malaria is so prevalent everywhere, that, although living in a very healthy quarter of the city, he *may* have had some contamination. But it is not at all likely that this, if he had it, would have given any indication of its existence, had he not subjected himself to the existing cause. It is proper to add that the opinion of the author does not coincide with that of Dr. Sims, who holds to the old theory.

The author was to-day applied to, as United States Examining Surgeon, by the Rev. B. F. Bowen, of Cold Spring, for a certificate, in consequence of a wound received in the attack on Petersburg in 1865. He was a chaplain in the army, but, moved by a patriotic impulse, he joined the ranks, and in a charge, and when stooping forward in the act of firing, he received a wound in the face and the shoulder. He says it felt at first as if a corkscrew were thrust into the part; then he was conscious of a terrible "concussion of the brain." He lay in front of the works from morning until night. He says that, at one time, during the probing of the wound, something like an electric shock passed from it towards the angle of the jaw, thence to the brain, and he became "*blind*" for an instant, but not syncopal or unconscious. Three times since then the same effect was produced by the same cause; when the probe reaches a certain point, at or near the bottom of the wound, or the non-fistulous tract, the electric-like sensation and the transient amaurosis occur. This operation was once done by a brother-in-law, J. W. Robathan, of Scranton; once by Dr. Stone, of Almont Mich., a former surgeon in the army, and once by Prof. Gunn, then of Detroit. The wound has affected him seriously, both

mentally and physically, and it is telling on his nervous system especially, more and more from year to year. Any attempt at manual labor affects his left arm and his brain. Sometimes, when he is preaching or lecturing, his mind, for an instant, or a few moments, is a blank, and his memory gone. The same occurs when reading or thinking. He has other singular sensations referrible to the region of the heart, which are, at times, still more alarming. These almost always occur while in the recumbent posture, and just as he is getting to sleep. A sensation like a powerful electric shock passes through the heart, and he leaps out of bed in a sort of fright and bewilderment. When in a strange house, it disturbs him so that he cannot sleep the remainder of the night. During a thunder-storm, the lightning seems always to go through his heart. At times the sensation, when lying down, is not so severe, and is like a vacuity or "goneness." There are almost constant twinges, and some soreness on the injured side of the neck, shooting up *superficially* towards the scalp, but entirely different from the alarming sensations above described, and which always appear to go deeply into the brain. Mr. Bowen's general appearance is that of fair health. He is a thoroughly educated, and a very intelligent and energetic man, and a man of great fortitude.

The ball entered the left side of the cheek just in front of the line of the masseter muscle, passed inside of the lower jaw, without injuring it, downwards behind the sterno-mastoid and the jugular vein, avoiding the carotid and other large vessels, very superficially towards the outer third of the clavicle, then between the scapula and ribs, to about the head of the lower third of the former, where it lodged. Six months after the wound was received, a soreness was observed at a point an inch or more above the junction of the outer and middle third of the clavicle, and an opening soon formed, from which two fragments of bone, from a half to three-quarters of an inch in length, were discharged. An opening still exists here, and discharges bloody pus, and sometimes, after exertion, blood. It has been a question, the patient says, among the medical men, whether the necrosed bone, which probably causes the symptoms, is the scapula or the rib. There is, I think, good reason to

suppose that it is the latter, and it will now be a matter for consultation whether an explorative operation ought not to be performed with a view to ascertain, and, if possible, remove the source of irritation of the nervous system, whether it be the bone or a bullet.

I have not the space at command to comment on the many interesting features of this case, as bearing on what has gone before; especially on the cases of amaurosis previously published by the author, to which I add the experience of M. Troussseau (Clinique Méd. de l'Hôtel Dieu de Paris), who speaks of temporary amaurosis caused by the tickling of worms in the intestines of children, a very common nervous symptom of their presence being also convulsions.

S. Weir Mitchell¹ records two cases similar in some respects to that here related. In one, the ball entered behind the ramus of the jaw, in front of the sterno-mastoid, and traversed the neck to the other side. Although there was no direct injury of the brain whatever, the man fell and remained unconscious for about half an hour. He then got up and marched off to join his regiment. I cannot here even epitomize the very interesting history of this case as given by Dr. Mitchell. He ends by saying: "In the case of Mooney, the symptoms consisted in loss of memory, vertigo, cephalalgia, and presenting such a degree of gravity as to necessitate his entrance into hospital long after the cure of the wound." Symptoms not very different from those of Mr. Bowen.

The other was the case of Captain, afterwards Commodore Stemble. Here the ball also took a course corresponding somewhat to one part of the tract of the ball in Mr. Bowen's case. Entering just above the os hyoides, it passed backward and downward, on the right side, under the sterno-mastoid, and came out near the superior angle of the scapula. Here there was temporary loss of vision in the right eye, slight ptosis remaining after two years, contraction of pupil, deficiency of sweat on right side, and exaggeration of this secretion on the left side of face and neck.

Dr. Mitchell remarks that there seems to have been some

¹ Des Lésions des Nerfs. Fr. Ed. Paris, 1874. Observation 58°.

disorder of the great sympathetic in this case; but that he is inclined to consider the symptoms as due to reflex action. Such is probably the explanation of Mr. Bowen's symptoms.



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